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THE MEDICAL JOURNAL OF AUSTRALIA

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VOL. II.—16TH YEAR. SYDNEY, SATURDAY, OCTOBER 12, 1929. No. 15.

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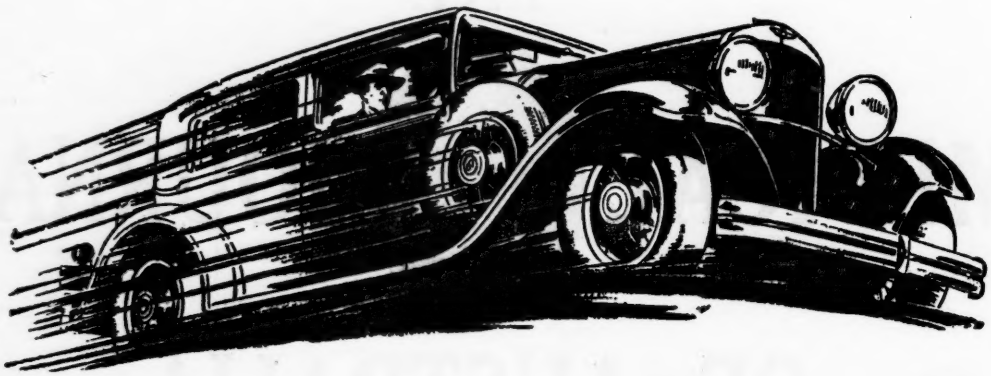

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Australasian Medical Congress (British Medical Association)

Sydney, 1929—*(Continued)*

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Feeding of Infants.

Dr. Vera Scantlebury (Melbourne) outlined a scheme to secure a recognition of the principles and practice of infant feeding. She suggested that the matter should be considered from the point of view of the medical profession, from that of the nursing profession and from that of the parents. The scheme had already been published in a booklet issued by the Health Department of Victoria. Dr. Scantlebury mentioned the contents of each successive chapter of the book. She also indicated in general terms the significance of the tables and methods appended to the reading matter of the booklet.

Dr. R. C. Everitt Atkinson (Perth) said that he hoped Dr. Vera Scantlebury had found that the members of the medical and nursing professions had sufficient intelligence to follow the details outlined in her scheme. Dr. Scantlebury's report should be of the very greatest value to those in charge of infant feeding clinics and infant welfare associations.

In opening the discussion Dr. A. P. Derham (Melbourne) said that he had been privileged in knowing the details of Dr. Scantlebury's work for some years and by reason of that more intimate knowledge he believed that her work as detailed in her pamphlet was epoch-making and would prove of national and international importance. For the first time in medical history there was a system available which would stand the test not only of clinical, but also of scientific accuracy. All her measurements had been checked by a highly skilled physicist and were reliable to a degree never before achieved. Some difficulty had arisen in Australia by the presence of two or more different schools of thought and of teaching in infant welfare. In Victoria, however, there was no real difference of opinion among competent pædiatricians as to the underlying principles of infant feeding, although amalgamation had not yet been achieved between the various organizations dealing with infant welfare. As far as artificial milk mixtures were concerned, the chief difference in principle and practice between the Plunket system and the system in common use in Victoria lay in the question of relatively low or high protein content in milk mixtures. In common with other Victorian pædiatricians Dr. Derham recognized the value and safety of the Plunket system of giving a low protein and high fat content. On the other hand some of the advocates of the Plunket system were not so charitable towards other schools of thought. They quite definitely condemned methods differing from their own and attributed the relatively high infant mortality in Victoria chiefly to the artificial milk mixtures used. In Dr. Derham's opinion this was a very serious fallacy. The excellent results achieved by Truby King in New Zealand and his followers all over the world were achieved by uniform teaching and practice in the use of safe and effective methods of infant feeding and mothercraft and not in the use of any one feeding mixture. There was an overwhelming mass of clinical evidence that artificial milk mixtures containing a relatively high proportion of protein were safe and successful in feeding large numbers of infants over long periods of time and there was no evidence that the method was harmful when properly used. The only biochemical evidence of the effects of high or even excessive proportions of protein in feeding young animals was that no kidney damage resulted in the presence of a sufficient amount of vitamins. These results were in accord with the clinical evidence of infant feeding.

Dr. Harvey Sutton (Sydney) suggested that the guide to infant feeding published by the Public Health Department of Victoria should be more advantageously put in the form of a pocket book. He thought the guide should be described as a scientific one and be better illustrated. He thought also that very thorough treatment should be given to the care of premature children. He mentioned the fact that in the United States of America he had seen rooms costing some three thousand pounds which had been designed to provide a uniform temperature for premature infants. He thought also that in a foreword to the guide greater emphasis should be given to the fact that breast feeding was the ideal and that artificial feeding should be resorted to only when the natural method of feeding failed. He believed that Dr. Vera Scantlebury's work was the best yet produced on the subject.

Dr. Guy Springthorpe (Melbourne) congratulated Dr. Scantlebury on her paper and the booklet on which it was based. Success in infant feeding could not be credited to any one particular method. General mothercraft methods and basic hygiene were the important things and he felt that wrangling over the various minor constituents of the mixtures was relatively unimportant. Dr. Grover Powers, of Yale, in 1925 had analysed the various mixtures used in infant feeding throughout the world. He had found that all the successful mixtures fell into one group with a small range in the percentage content of protein, carbohydrate and fat and in consequence in the caloric values. All other differences were of dilution or the use of different ingredients. He thus showed that differences in various mixtures were more apparent than real.

Dr. E. Sandford Morgan (Sydney) referred to previous speakers who had remarked that breast feeding was all important and pointed out that there was a very comprehensive scheme of breast feeding in the guide. It was assumed that everyone present realized the importance of breast feeding and she assumed that this was the reason why Dr. Scantlebury had dealt mainly with artificial feeding. She did not think that breast feeding had been pushed into the background in any of the States and she thought that the pages on breast feeding in the guide were the best she had seen.

Dr. W. C. Petherbridge (Sydney) said that Dr. Vera Scantlebury had offered a good variety of emulsions. It was comforting to have such a comprehensive work to which reference could be made. He doubted the possibility of having such a scheme as that suggested by Dr. Scantlebury adopted throughout the country, as busy practitioners and people working in health centres *et cetera* had little time for consulting literature during the course of their work. He hoped that a uniform scheme would eventually be adopted, not only in one State, but through the whole Commonwealth. If such a scheme were ever instituted, he was sure that Dr. Scantlebury's work would largely be concerned in it. A lack of harmony existed between the general practitioner and the infant welfare workers. This was largely due to disagreements in regard to the methods of preparing infant foods. He stressed the fact that the promotion of infant welfare was of far greater importance than curing the infant after it had become ill.

Dr. J. Dale (Melbourne) noted that one speaker had said that 75% of infants in New South Wales were breast fed. If such were the case he felt he should congratulate New South Wales, as certainly Victoria was not nearly so fortunate. He felt the medical profession had failed owing to differences of opinion in infant feeding methods.

Sir Truby King (Wellington, New Zealand) congratulated Dr. Vera Scantlebury on her paper and the vast amount of work which she had done. A list such as hers was of great value. He questioned the desirability of bringing together all the different methods of infant feeding without expressing any definite opinion on any one method and leaving the choice to the medical man himself. He found himself entirely at one with Dr. Harvey Sutton who placed the whole matter in a nutshell when he said that all forms of infant feeding should be based on the natural feeding by the mother. Such was the basis on which he had commenced his campaign, but it was only after fifteen years of study in the feeding of the mammalian young generally. The amount of scientific work and investigation into the feeding of domestic animals had been much greater than had been the case in human beings. For very many years the correct feeding of cattle, pigs and sheep had been exactly known throughout the world and there were many complete and elaborate tables laid down for the use of agriculturists. Opinions were

unvarying in that the young of an animal should be fed according to the milk of the mother. The greatest authority on nutrition in Europe, von Pirquet, had laid down the rule that 1·5% of protein was the optimum in the feeding of infants. It had been suggested that he was opposed to high protein feeding. Actually he had never expressed this opinion, though in general he believed that the percentage of protein should be about 1·5%. He asked if the medical profession was justified in saying that cane sugar could be safely substituted for milk sugar, while the latter had been introduced by Nature and the former by the work of man. He wished to emphasize this point on account of recent investigations which went to show that milk sugar was concerned very intimately in the development of nerve and brain. It was rather too much to say that in New Zealand they advocated only one method of feeding. Certainly one particular method was used, but this was for the purpose of obtaining uniformity.

Dr. Vera Scantlebury in reply said she realized the difficulty that would be experienced by busy practitioners in using the guide book and for this reason an abridged edition had been made in hand book size; unfortunately this was not quite ready. She thought that the matter of breast feeding had been made fairly prominent in the book; it had been mentioned in the foreword and the first chapter was devoted to it. She wished to express the gratitude she felt for the information she had gained in the Karitane hospitals in New Zealand. She was definitely of the opinion that milk sugar was preferable to cane sugar. She hoped that if the system could be further used, it would eventually spread over the whole Commonwealth. A great advantage would be gained through teaching uniform methods to medical students.

Scarlet Fever and Diphtheria.

A combined meeting of the Section of Preventive Medicine and Tropical Hygiene and the Section of Medicine was held on September 4.

Before calling upon Dr. Scholes to read the first paper DR. R. C. E. ATKINSON said that epidemics of diphtheria and scarlet fever still occurred despite the methods which had recently been devised for the prophylaxis and cure of these diseases. In the past there had been difficulties in the way of gaining control over scarlet fever and diphtheria through the differences of opinion between the medical practitioner and the health officer. They had an opportunity of discussing the question and he hoped that many of the difficulties and differences of opinion might be dissipated.

DR. F. V. SCHOLES (Melbourne) discussed several problems in connexion with scarlet fever and diphtheria. He said that it was useful to examine the newer knowledge of scarlet fever and that this knowledge would throw additional light on the comparative failure of the management of diphtheria. In addition to the classical picture of scarlet fever they had to recognize *scarletina sine eruptione*, chronic or intermittent infections, temporary carriers showing no signs of infections. *Streptococcus scarlatinæ* was the infecting agent and the criterion of infection was the presence of the *Streptococcus scarlatinæ*. This was probably not a specific entity, but neither was the *Bacillus diphtheriæ*. Any hæmolytic streptococcus which was agglutinated by a standard antitoxic serum, could for practical diagnostic purposes be regarded as *Streptococcus scarlatinæ*. It was one species with common capabilities, producing one toxin with divers potentialities and one antitoxin capable of neutralizing all toxic manifestations. After the establishment of the ætiological rôle of the streptococcus the next advance had been the production of an antitoxic serum. He had obtained quite definite results from using serum. He had found a rapid improvement in the patient's condition, provided the serum had been given early, a fading of the rash, a lack of desquamation, a sharp fall in temperature, unless some obvious and gross local inflammation or general sepsis were present. The results were strictly comparable with those obtained in diphtheria. It had become apparent that not only did serum fail to influence the course of septic complications present before the serum was given, but the late troubles of convalescence were more common than in the preantitoxin days. This impression had gradually become stronger. It was necessary to make certain whether in children treated early with serum there was a greater tendency to lose their immunity and to lose it more rapidly than those who were allowed to make their own fight. Dr. Scholes went on to discuss the Dick test and second attacks of scarlet fever and said that what was required was some means of causing the streptococci to disappear from the mucous membrane. The question could thus be compared with that of diphtheria; the position was the same and the problem was the same. Was it not possible that children recovering as a result of treatment were by the early induction of passive immunity deprived of the stimulus to antibacterial action? In diphtheria the position was somewhat different from that in scarlet fever, for the *Bacillus diphtheriæ* was practically a saprophyte living on the surface of the body and, once a state of toleration developed, there was little inducement for battle on either side. What was wanted was a potent serum, combining antibacterial or opsonic with antitoxic qualities. In the case of scarlet fever this might not be long delayed, but the position in regard to diphtheria was not so hopeful. Dr. Scholes then referred to the treatment to be adopted in the presence of scarlet fever. He said that the non-susceptible and non-infected could be ignored, the non-susceptible and infected should be isolated, the susceptible and infected should be isolated and treated with antitoxin and the susceptible and non-infected should be immunized. They were a long way from their ideal. They did not want merely to render a child non-responsive to the Dick or Schick test; they wanted to make him and keep him uninfected. A purely antitoxic immunity could be maintained only by a ceaseless struggle. Beyond all this they had to look towards the prevention of unhealthy conditions and to the factors which were making chronic infections of the upper respiratory tract a menace to national health.

DR. D. GIFFORD CROLL (Brisbane) submitted the result of an analysis of the treatment of children in the Children's Hospital, Brisbane, suffering from diphtheria and its results. On the basis of this inquiry he endeavoured to find a reply to three questions, whether swabbing of throats was of value in diagnosis, whether advantage could be derived from the exhibition of large doses of antitoxin and whether patients with diphtheria should be given antitoxin before being sent to hospital. The material comprised three hundred patients. Of these 294 recovered, six died. The mortality was therefore about 2%. Four patients were hopelessly ill at the time of admission; two might have been saved, had larger doses of antitoxin been given. The disease had appeared three days before admission in two patients, four days before admission in one and more than four days before admission in three patients. The duration of the disease at the time of admission had been one day in eighty-four patients and two days in fifty-five, making one hundred and forty-one. None of these children had died. In sixty children the duration on admission had been three days, in four it had been twenty-one days and in sixty it had been more than four days. Among the one hundred and forty-one patients the mortality had been 6%. On one occasion the practitioner had awaited the result of the bacteriological examination of the mucus on the swab from Tuesday till Thursday before injecting antitoxin. In the meantime the child had become very ill. It had died on the twenty-eighth day of diphtheritic paralysis. Swabbing of the throat had been taken under advantageous conditions and the bacteriological examination carried out in the laboratory of the Brisbane and South Coast Hospitals Board. The bacilli had been grown from the mucus of 164 patients out of three hundred at the first examination. The bacilli had not been grown from the mucus of 104 patients either at the first or at subsequent examinations. He therefore concluded that swabbing as a diagnostic expedient was quite unreliable. The type of disease was faucial in 162 patients, nasal in thirty-two, laryngeal in eighteen, faucial and nasal in ten and faucial and laryngeal in seventy-eight. The dose of antitoxin injected varied between two thousand and over one hundred thousand units. The majority received between thirty and sixty thousand units. The

complications numbered seven, palatal paralysis twice, *otitis media* three times, pyelitis twice. Intubation had been required twelve times and tracheotomy four times. The membrane disappeared from the throat in 2·1 days after twenty thousand units of antitoxin, in 2·8 days after thirty thousand, in 2·8 days after forty thousand units, in 3·1 days after fifty thousand units and in 3·9 days after sixty thousand units. From the information at his disposal Dr. Croll concluded that it was wiser not to administer antitoxin before the patient was sent to hospital. The risk to a patient travelling a long distance immediately after having received a dose of antitoxin was very great. He preferred to delay the administration rather than to increase the risk during the transport.

Dr. J. Dale (Melbourne) read a paper on the prevention and control of diphtheria and scarlet fever. He pointed out that certain infective diseases had been controlled on a large scale. Diphtheria and scarlet fever might have been controlled if these infections had been a greater danger to the public welfare. In discussing the parasite of infective diseases, he distinguished the invasive power of the organism from the toxicity. It had been found that the invasive power of the diphtheria bacillus was dependent on its numbers. The body offered general, natural or innate resistance to an organism and a specific resistance directed to a particular organism. Turning his attention more particularly to the two diseases, he found certain similarities. The incidence was greatest among young children. Both were due to the growth of the infecting agent on the mucous membrane of the throat and occasionally other sites. The general symptoms were caused by an exotoxin circulating in the blood. These exotoxins could be prepared *in vitro* and antitoxins could be called into existence and used therapeutically. The organisms of the two diseases belonged to a genus of which numerous species or varieties were apparently saprophytic. Toxigenic organisms were highly specific and world-wide in distribution. The proportion of carriers of diphtheria bacilli varied from 66% among immediate contacts to 0·1% among the general population. There seemed to be wide variations in regard to the virulence of the bacilli found in the throats of carriers. While the position was not clear in regard to diphtheria bacilli, it was much more obscure in regard to the hæmolytic streptococcus of scarlet fever.

The chief measures for the control of diphtheria were the isolation of patients and the search for carriers with a view to their isolation. The occurrence of mild infections and the great prevalence of carriers among contacts rendered these measures at times of little value. Under certain circumstances wholesale swabbing led to the discovery of the sources of infection. Dr. Dale suggested that testing by Schick's method and active immunization had rendered the effective control of diphtheria possible. The test served to discover the susceptible; these children could be immunized and the immunity was of long duration. The disease could be banished if it were worth while. He held that it was worth while in certain institutions, such as schools, children's homes and hospitals. A campaign of Schick testing and immunization had been carried out in Melbourne from the end of 1925 and had led to the raising of the herd immunity of the school children. But it had been found that 63% of the deaths from diphtheria occurred among children below school age. Dr. Dale advocated general immunization in infancy without Schick testing which had been proved to be unnecessary at this age.

The control of scarlet fever had been limited to the isolation of patients and possible contacts. Dick testing and immunization had been used extensively in nurses of fever hospitals. A certain amount of work had been carried out in Melbourne. While the results seemed to be satisfactory, the disease itself was so mild that it would not be worth while to adopt general measures to control the disease. Dr. Dale appended to his paper a short account of the work carried out in Melbourne in the endeavour to control both diphtheria and scarlet fever.

Dr. Helen Kelsey (Melbourne) read a paper in which she discussed clinical laboratory work in connexion with diphtheria and scarlet fever. She divided this work into the examination of smears, the examination of swabs, testing for virulence and the use of the Schick test and immunization. In the examination of smears in diphtheria there was such a large margin of error (40%) that they should under no circumstances be relied upon. There were certain errors connected with the examination of swabs. Sometimes a large amount of mucoid exudate interfered with the growth of the bacillus. Another source of error was inefficient swabbing. The membrane might be streptococcal or the condition might be due to Vincent's angina or to syphilis. Tonsillitis might occur with non-virulent diphtheria bacilli. In examining swabbings from other parts of the body and the throat, the bacteriologist should have all available information in regard to their source. In these circumstances the ultimate test was that of virulence. As the tests for virulence were practised no definite distinction was made between virulence and toxicity. These did not necessarily run parallel. Dr. Kelsey gave details of a case in which the organism had a low virulence and a high toxigenic power. Further research was needed in this matter. Turning her attention to scarlet fever, Dr. Kelsey said that the hæmolytic streptococci from patients at the Infectious Diseases Hospital, Fairfield, almost invariably fell into three groups according to their macroscopical appearance. She described these colonies and said that colonies of similar appearance might be found in convalescent diphtheria patients, in those suffering from tonsillitis, erysipelas and puerperal fever and in apparently normal throats. The work of Griffiths had shown that hæmolytic streptococci in scarlet fever tended to fall into four main serological groups and a fifth heterologous group. The infections caused by Types I and II were severe and gave rise to complications, that of Type III was moderate and Type IV infection was the mildest. Opposed to these observations McLachlan and Mackie had not been able to come to a satisfactory conclusion. After referring to some of the difficulties in connexion with the Schultz-Charlton test and pointing out that further investigation was required, Dr. Kelsey said that there was a number of patients with typical infections who did not react to the Dick test. There were persons with the brightest rashes. Perhaps the capillaries were dilated to their utmost and no further erythema was possible. Although the Dick test was neither so simple nor so constant as the Shick test, assistance could certainly be obtained in the diagnosis of doubtful infections if plate cultures, the Schultz-Charlton test and the Dick test were used. Dr. Kelsey went on to compare the results obtained in patients treated with antitoxin and in those treated without its help. She illustrated her remarks by tables. In those treated without antitoxin the number of reactors to the Dick test was high in the first week and this number dropped in the second, third and fourth weeks and rose slightly in the fifth and sixth weeks, but to an extent comparable with the patient's treatment with antitoxin. Amongst those treated with antitoxin the number of reactors in the first week was low and a gradual rise occurred. These figures appeared to support the idea that relapses or second attacks were more common after antitoxin treatment. In regard to the discharge of patients it was necessary to rely on the appearances of colonies from swabs on account of the magnitude of the labour involved in culturing and typing the organisms. She had found no relationship between the persistence of streptococci in the throat and the Dick reaction. Tonsillectomy in persons with persistent organisms in the throat yielded better results in scarlet fever than in diphtheria.

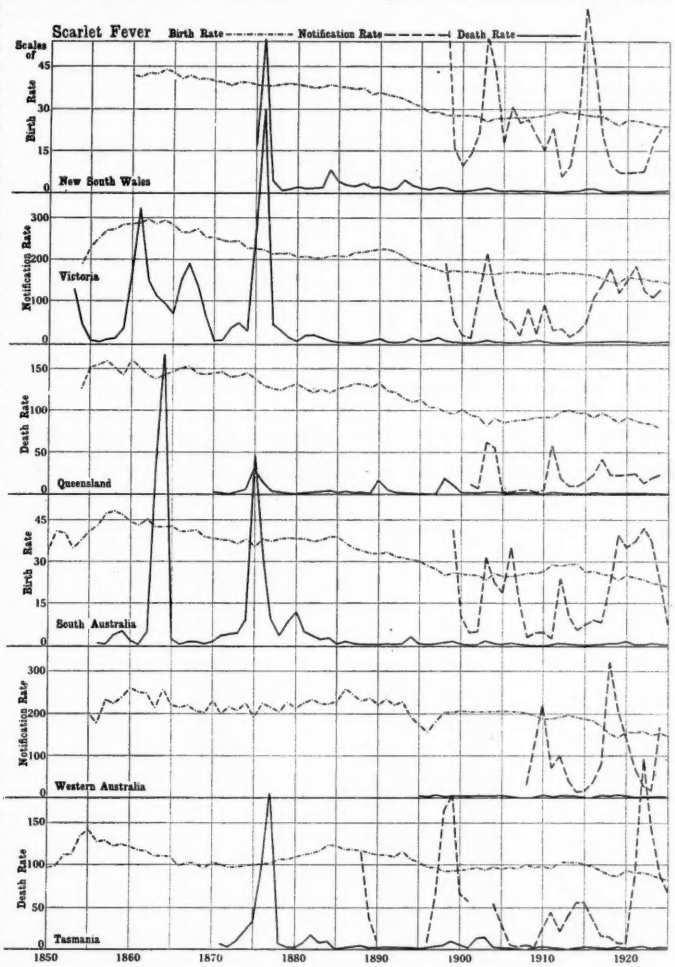
Dr. F. G. Morgan and Dr. C. W. Adey (Melbourne) gave information concerning the modern methods of determining and abolishing susceptibility to diphtheria and scarlet fever. In carrying out the Schick test the toxin had to be fixed in terms of minimum lethal doses for a guinea-pig weighing 250 grammes. One-fiftieth of the minimal lethal dose diluted to 0·2 cubic centimetre was injected intradermally. It had been found that allowance had to be made for variations in toxin and for the elimination of certain discrepancies in the results. A definite standard had been introduced in Great Britain and it had been possible to compare the strength of the various samples of toxin with this standard. Suitable toxins had thus been selected. Recent work had revealed that the best immunizing agent was anatoxin, a toxoid preparation made

by adding formaldehyde to a culture of diphtheria bacilli in broth. The filtrate had been found to be non-toxic and yet to have retained a very high proportion of its antigenic value. Anatoxin was superior to toxin-antitoxin mixture and to anatoxin-antitoxin flocculated precipitates. Some individuals, however, proved to be sensitive to anatoxin, although the experience of its application in Russia had shown that fatal results had not been observed. The authors held that it was advisable to test for anatoxin sensibility before proceeding to inject the substance for the purpose of immunizing the individual. The test was carried out simultaneously with the Schick test. If the Schick test failed to induce a reaction, indicating susceptibility to diphtheria, a response to the anatoxin test indicated the need for caution. The author suggested that the sensibility to anatoxin probably depended on some degree of latent immunity not revealed by the Schick test. They were prepared to refrain from further injections of anatoxin in these persons. The majority of individuals who failed to react to the Schick test, tolerated anatoxin well and could be immunized without any danger. As a rule two injections of 0·5 and 1·0 cubic centimetre respectively were given. The authors held that a third dose could be given with advantage, although they admitted that it would be advisable to restrict the immunizing to two injections. About 90% to 95% of the persons injected with anatoxin lost the power to react to the Schick test and thus presumably their susceptibility to the disease.

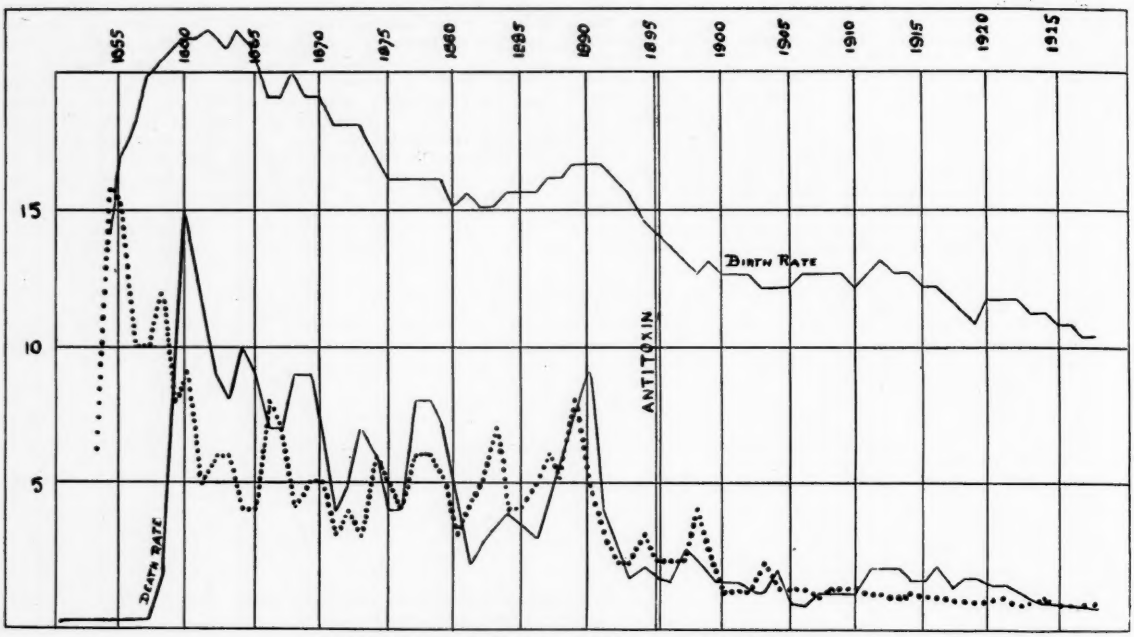
In regard to the application of the Dick test for scarlet fever and to the process of immunizing susceptible persons with streptococcal toxins, the authors found that some difficulty was encountered in the determination of the potency of the toxin and antitoxin. It had been found that Saanen goats were suitable for the application of the intradermal test. In Melbourne they had used Angora goats with moderately good results. The toxin proved to be unstable, both when undiluted and when diluted. The authors discussed suitability of various types of toxin both for testing purposes and for immunization. They used Dochez and Fairfield strains for both purposes. Streptococcal antitoxin prepared from horses with the aid of Fairfield strains blanched the rash at the Fairfield Hospital only. This implied that a Schultz-Charlton reaction was obtained.

Dr. R. J. Millard (Sydney) said that in diphtheria sometimes the bacteriologist reported that the infection was due to a non-virulent type of bacillus. What guarantee was there that such a bacterium would continue to remain non-virulent? He thought that it was desirable that each patient with scarlet fever should be examined for the presence of hæmolytic streptococci, but in a large hospital this was impracticable owing to the great numbers of scarlet fever sufferers who were admitted. He hoped that the time might come when some simple examination such as was conducted in the case of the Klebs-Löffler bacillus should be possible. He mentioned the value of the Schultz-Charlton reaction in the differentiation of scarlet fever and quoted an illustrative case which he had recently observed in the Coast Hospital.

Dr. J. H. L. Cumpston (Canberra) said that before they began to form conclusions as to the possibility of controlling a disease, they should know as much as possible of the natural history of the disease and of the course it would follow if left uncontrolled. They should not be dogmatic even on the historical aspects, but should at least look at the course of the disease. A dramatic period in the story of the epidemics of the world was the sudden appearance of diphtheria all over the globe in the years 1858 and 1859 in Europe, America, North Africa, India, China and the Pacific. Of its novelty to nearly the whole British profession in 1858, familiar as they were with the angina of scarlet fever, there can be no question, even in the face of Jenner's assertion in 1861 that he had seen this disease occasionally as long as he had been in practice. Its first appearance in Australia in 1858 was very definite and unmistakable. In Victoria the statistical records had been more complete and continuous than in other countries. The graph of the death rate presented showed the abrupt rise through the years 1858 and 1859 to the great climax of 1860. Thereafter there was a rapid fall with occasional epidemics to a minimum in 1881.

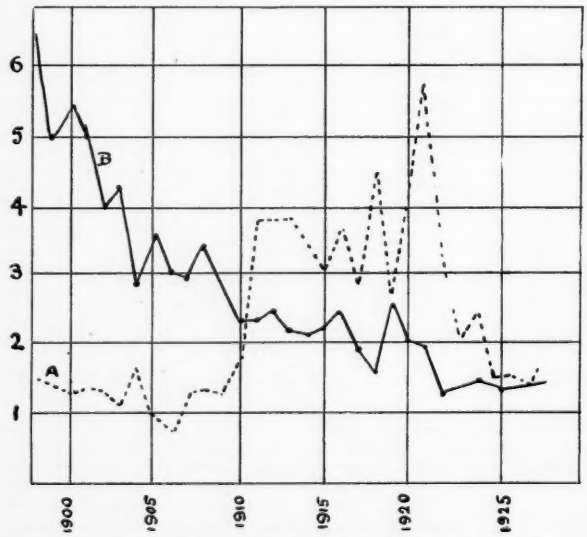


In the years 1888, 1889 and 1890 there was a notable epidemic exacerbation after which the death rate resumed the course it had previously been following. First should be noted the uniformity of the fall in the death-rate, a uniformity not certainly affected by the use of antitoxin if the mere form of the curve be considered. It may have been that antitoxin prevented the death-rate from rising again, but no one can assume the probability of the occurrence of certain events which in fact did not happen. They were faced with a disease showing a very definite inherent tendency to decline even before the introduction of a specific remedy. The next point was the relation between the birth-rate in the community and the death-rate from diphtheria. The great immigration into Victoria in the decade 1850-1860 resulted in the abnormally



Death Rate from Diphtheria in Victoria per 100,000 living.

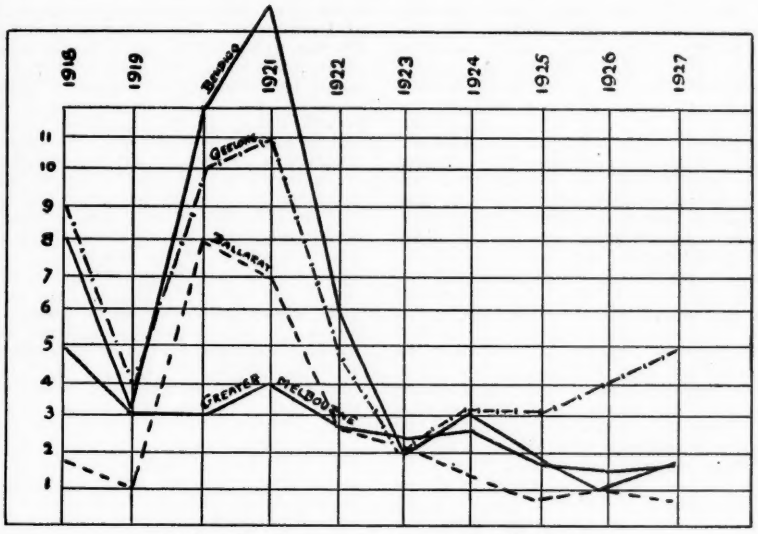
high birth-rate between 1855 and 1870. This accumulation of susceptible material occurred simultaneously with the great rise in diphtheria death-rate. Between 1885 and 1890 the great immigration of the 'eighties produced an increased birth-rate; simultaneously a sharp rise in the diphtheria death-rate occurred. The same sequence of events, although in lesser degree, had been seen between 1910 and 1915. It thus appeared that certain factors assisted to determine the course of an epidemic disease and to affect the prospects of success of human effort. The course of the death rate since the introduction of antitoxin suggested some question as to the exact place of antitoxin in the control of diphtheria as an epidemic rather than as a disease of the individual. From the graph of the notification rate and that of the case mortality rate it was to be observed that while the case mortality rate had fallen steadily since 1900, the period 1910 to 1925 had been characterized by a great increase in the relative prevalence of the disease. It was clear, therefore, that the increased use of antitoxin had not prevented the epidemic recurrence of the disease. There were certain indications that the virulence of the disease was increasing and also that individuals were failing to react to antitoxin. The history of diphtheria in relation to antitoxin pointed to the necessity for accepting with great caution any conclusions based upon the behaviour of the disease over short periods. The control of the disease was well discussed in the memorandum by the Ministry of Health (1922) and in the volume issued by the Medical Research Council (1923). It was difficult to add anything to these official pronouncements. The principal features could be indicated. The control of a disease such as diphtheria depended upon the practical segregation of the known infective person and steps aimed at controlling infection from unrecognized sources.



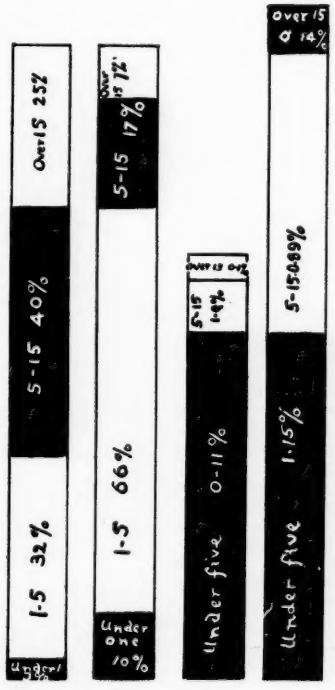
A = Notification Rate per 100,000 living—Diphtheria in Victoria.
B = Case Mortality—Diphtheria in Victoria.

Before the control of the infective person early knowledge was necessary and the established procedure of notification, treatment in bed under complete isolation and the use of such remedies, specific and otherwise, as would bring a patient to a cure rapidly. There had been much wholesale swabbing carried out in the past, with the consequent discovery of hundreds of persons with diphtheria or diphtheria-like organisms in the nose or throat. These presented an administrative problem of considerable magnitude, but unless the detection of carriers was accompanied by determination of virulence, it was valueless. The probability of the infective agent in a convalescent or the close contact of an actual patient being virulent was sufficiently great to justify special attention to these, but the nature of this attention was as yet doubtful. The records of success of wholesale immunizing of a community were somewhat incomplete. The most completely controlled example was that of the Naval School at Greenwich where Commander Dudley had been able

to satisfy himself that diphtheria had for at least one year been controlled by active immunizing of the whole muster of boys attending the school, but these records were available only for 1928 and it could not yet be said whether this represented a permanently successful method. This method as applied to a community involved certain considerations. The age distribution of patients with diphtheria indicated that the pre-school age was the most important. This was the age of maximum susceptibility, according to Park, as shown by Schick. On the other hand, it was possible that as the school acted as an infection exchange, attention to school ages was administratively most important. It was doubtful whether any legislature would make immunization compulsory and as all work done must be on a voluntary basis, it was evident that the whole population would not be reached. In any event a universal campaign would be costly and would need to be carried on continuously. The English experience showed that even with the attention paid to vaccination against small pox, the disease had not been eradicated. Compulsory vaccination had not been maintained, with the result that a total of 14,767 cases of small pox occurred in England in 1927. There was also the interesting example of South Africa where the inoculation of native workers in the mines against pneumonia appeared for some years to be successful, but later had failed, a new type of organism or modified variant of the original organism causing as much damage as originally. Some attention might also be given to the phenomenon established by Dudley that during a school epidemic of diphtheria a number of susceptible boys determined by the Dick test became immune without any appreciable attack. Two observations by Dudley as a result of his work at Greenwich were important. If the dose of diphtheria bacilli were sufficiently large and injected into the peritoneal cavity instead of into the subcutaneous tissue, both the virulent and avirulent varieties could kill guinea-pigs even when they were protected with diphtheria antitoxin. Hundreds of strains of bacilli had been studied; the results had not been completely correlated with the other observations. They give a hint that long period fluctuations in the characters of the parasite might be more important in the causation and periodicity of outbreaks than was generally admitted. It was known that



Notification Rate per 10,000 living—Diphtheria.



For Year 1921—New South Wales Diphtheria.

A. Percentage ratio each age period of total cases notified.

B. Percentage ratio each age period of total deaths.

C. Rate per 10,000 living in age period of deaths in that period.

D. Rate per 10,000 living in each age period of cases notified in that period.

diphtheria could be controlled in the individual by this method and it was within the province of every practitioner to secure immunization of the susceptible children among his patients. The position could be summarized by stating that for the control of diphtheria certain lines of action were indicated. Although elements of uncertainty associated with these lines existed, they presented some promise of success and until better methods were available, these must be adopted. The most important was the early detection, isolation and treatment of the patient, accompanied by control of environment preventing insanitation, respiratory overcrowding and undernourishment.

The natural history of scarlet fever was an important factor in any discussion of the control of this disease. It assumed a high degree of virulence in epidemic waves at intervals of thirty to fifty years. In the middle of the last century a degree of virulence of scarlet fever had been encountered beyond anything familiar to present practitioners, so much so that in 1875 a day of prayer had been ordered in the churches of Melbourne on account of this disease. Since then it had become almost negligible and apart from its renal and ordinary complications might be regarded as a minor disease. For its control practically all that could be said, was that the known patient should be isolated either in hospital or at home as early as possible and kept isolated until mucous discharges had ceased. The value of immunization was even more uncertain than that of diphtheria and probably practitioners would have some hesitation about recommending repeated inoculation for immunizing against scarlet fever with the present condition of virulence of this disease.

The Health of the White Man in North Australia.

In a paper by Dr. C. E. Cook (Darwin), read by Dr. E. S. Morris, the author discussed the health of the white man in North Australia. He said that the opinion was still held that climatic conditions in tropical Australia were inimical to successful settlement by a healthy white-skinned race. This view was commonly supported by reference to the wet bulb temperature. Malaria, filaria and other causes of chronic morbidity were rarely put forward as adverse factors. Dr. Cook referred to the wet bulb temperatures at Darwin. The physiological limit for work in

stagnant air, 27·2° C. (81° F.) had been reached on thirty-two occasions during the hottest months of the period 1928-1929. It had been exceeded on sixteen occasions. Although it would appear from this that the employment of white labour in such a climate was a physiological blunder, it was a fact that the limit of 27·2° C. had been set as a result of experiment under artificial conditions with subjects unused and unadapted to other than temperate climates. The standard was not applicable to field conditions. At Darwin the period of high wet bulb temperature coincided with that of the north-west monsoon and the condition of stagnant air was not reproduced. At the same time the residents were for the most part acclimatized and all were suitably clad. A wet bulb temperature of 31·1° C. (88° F.), tolerated for muscular effort in moving air, would be a more apt basis for comparison and even Darwin's worst figure was well below this. Dr. Cook also referred to work undertaken by men of the Navy in scraping and painting the interior of tanks during the heat of the day. None of them had manifested any impairment of efficiency or become ill. Furthermore, the youths of Darwin always played football during the hot season in the heat of the afternoon. There was no evidence that the climate had any deleterious effect on the pregnant female. When the weight of all infants at each quarter of the first period of life was compared with that of Victorian infants at the same period, it was found that the infants in North Australia were heavier at each period. The infantile death rate for North Australia was 2·0 per hundred live births, while the figures for New South Wales and the whole of Australia were 5·7 and 5·6 respectively. When allowance was made for the fact that the bulk of the white people in North Australia was adult males, the death rate of 13·5‰ was in striking contrast to the rate of 33·6‰ for the male population of New South Wales, including age groups over twenty. Dr. Cook emphasized the fact that the successful white settlement of North Australia would be contingent upon the community dealing successfully with the problems of hygiene. He then gave an outline of the activities of the North Australia Medical Service. The real dangers associated with the endemicity of certain diseases were recognized and steps were being taken to prevent further introduction and to eradicate existing sources of dissemination.

Dr. R. C. E. Atkinson (Perth) mentioned the fact that he valued Dr. Cook's opinion and the care with which he made his observations. One of the greatest difficulties in public health matters in northern Australia was the regular inspection of the widely scattered population. The Commonwealth Health Department was making an endeavour to obtain more adequate health control over this area. He hoped that it would not be long before the whole area would be regularly patrolled by a medical officer for the detection especially of leprosy and venereal diseases. He had noted that children as a rule did very well in northern Australia and appeared to escape many of the diseases of temperate climates. He suggested that such children might be very susceptible to the infectious diseases when they visited the southern States. He thought, however, that white women who were called upon to bear children in tropical Australia did not stand the strain well unless they were able periodically to visit a temperate climate.

Dr. Kate Knowles (Sydney) stated that she had heard Dr. Cook speak before on this subject and she feared that he was perhaps somewhat biased, as he was very keen on his subject and keen to prove that northern Australia was habitable and healthy for white people. She suggested that the high incidence of deaths from cerebral hæmorrhage might be due to the high temperature and that the high rate of deaths due to cancer might be put down to the prevalence of skin cancer. She thought that Dr. Cook's statistics were not sufficient to show whether or not such was the case.

Venereal Infection in Girls.

Dr. K. S. MacArthur Brown (Sydney) read a paper in which he dealt with the incidence, diagnosis and control of girls committed to institutional care. He based his remarks on work carried out at the Girls' Industrial School at Parramatta. The histories of 650 girls were analysed. Of this number 75% were bright mentally, 24·5% had a mentality which might be described as dull, and 0·5% were mentally deficient. Twenty of the girls had been *virgines intactæ*. Of the 630 *non-virgines intactæ* 46 had been pregnant, 19 had been parous, seven had been married and 26 had been prostitutes. The ages of the girls ranged from ten to seventeen. After describing the routine method of examination, Dr. MacArthur Brown said that 33% of the girls had been found to be suffering from gonorrhœa, 5% had syphilis and 29% were affected by an inflammation of non-specific origin. He had found the complement fixation test to be highly specific for gonococcal infections. The reaction had been obtained in 94% of proved gonococcal infections and it had been persistently absent in girls who were known to be healthy. It had also been found that there was a gradual disappearance of the reaction during treatment, the weakening of the response being accompanied by an improvement in the clinical condition. The treatment of gonorrhœa was divided into three stages. In the first stage the urethra was irrigated with permanganate of potash and the uterine canal was swabbed with "Flavine" as far as the internal *os*. In the second stage the urethral canal was dilated with Hegar's dilators and painted with a solution of silver nitrate. In the third stage the cervical canal was painted with iodized phenol and the swab stick was passed just into the uterine canal. If there was any difficulty in passing the stick through the internal *os*, it was a sign that the cervix was not seriously implicated. After describing the clinical manifestations of syphilis that had been encountered, Dr. MacArthur Brown said that the flocculation test was more delicate than the Wassermann test, because it would give a stronger reaction than the Wassermann test in badly infected patients and would be the last to disappear in the course of treatment. A full course of treatment consisted of twelve intravenous injections of "Novarsenobillon," one injection being given every week, a mixture containing iodide of potash and perchloride of mercury for two months, a second course of twelve injections of "Novarsenobillon" and a second period of four months' treatment with an iodide of potash and perchloride of mercury mixture.

Dr. R. C. Everitt Atkinson (Perth) wished to congratulate Dr. Macarthur Brown on the thoroughness of his investigations. Dr. Brown had had an extraordinary opportunity of treating gonorrhœa and syphilis in females and he had placed before the section a record of the excellent work he had done during the past four years. He had been interested to hear the results of the complement fixation test in gonorrhœa, as observers recently had thrown doubts on its value. He asked Dr. Brown why no mention had been made of mercury in the treatment of syphilis.

Dr. R. V. Storer (Sydney) said that Dr. Macarthur Brown's records should be of very great value to people who treated venereal disease in women. He wished to ask at what period in the course of gonorrhœa did the reaction to the complement fixation test appear. In his own experience no reaction was obtained in the early stages. Dr. Macarthur Brown had found that the gonorrhœal infection in the majority of his patients had taken five months to clear up. With a similar technique, save that methylene blue alternating with "Neo-protargol" instead of "Flavine" was used, and the same standard of cure, he found that the infection in a subacute stage could be cleared up in two or three months. He thought that methylene blue had a specific action against the gonococcus and might assist in the development of the gonophagic substance. He had been surprised to note that Dr. Macarthur Brown had not mentioned the use of bismuth in the treatment of syphilis. Certain investigators had recently shown that "Neo-salvarsan" destroyed spirochætes in four hours, bismuth in twelve hours and mercury never. He believed that bismuth would ultimately replace mercury and was sure that it had a powerful spirillicidal action.

Dr. Marie Hamilton (Sydney) congratulated Dr. Macarthur Brown on his paper which she felt must have involved a great deal of work. She had done some work in conjunction with Dr. Elsie Dalziell and Dr. Brown. She was of opinion that medical practitioners did not take

sufficient interest in gonorrhœa and that there was great room for improvement in its treatment. The complement fixation test appeared to her to give very encouraging results in her work in the Health Department. She had had an unusual opportunity of estimating the value of the test, as she was dealing with gonorrhœa in the inmates of the Girls' Industrial School and the question of reinfection did not arise. Complement was usually not deflected in the early stages, though she had known reactions to appear in patients who had suffered from the disease no longer than two days. The average period at which a reaction was obtained was about twelve days from the commencement of the disease. In contrast to Dr. Brown's experience she had not encountered frequent infection of Bartholin's glands in her clinic at Redfern. Non-specific leucorrhœa was a matter which should be carefully looked into. This condition was common in her clinic and she thought that vaginal douching might be concerned in its ætiology. Dr. Brown had stated that primary chancre was rare in the Parramatta institution; in her clinic it was frequently seen. In the diagnosis of primary syphilis she obtained good results from staining smears by Fontana's method.

Dr. J. Cooper Booth (Sydney) said that he had visited the Girls' Industrial School at Parramatta and had been very impressed with Dr. Brown's work. The massive doses of arsenical preparations used there surprised him at first, but he had observed that the girls were apparently very healthy and appeared to do very well. It was a pity that the guardianship over these girls did not extend beyond the age of eighteen. After this age the girls on their discharge from the institution occasionally became pregnant and contracted venereal disease. It was unfortunate that there was no suitable place in which then to attend to these people.

Dr. Macarthur Brown in reply said, although no mention had been made in his paper of the use of mercury in the treatment of syphilis, he was in the custom of giving *liquor hydrargyri perchloridi* as a routine. He had tried mercury by inunction, but had not been convinced of its value and had observed that it was apt to give rise to abscesses. He gave bismuth by injection as a routine, as no reaction followed its use, but he had never used the drug alone in the treatment of an infection. A complement fixation reaction was not always obtained when the patient first came under control. Non-specific inflammation as mentioned by Dr. Marie Hamilton, was of particular interest and its ætiology was still obscure. Often it very closely resembled acute gonorrhœa. Only recently he had seen a young girl of fifteen who was suffering from a condition which in the absence of laboratory evidence he felt sure would have been diagnosed as acute gonorrhœa; actually no gonococci were revealed at an examination of a vaginal smear. He realized the importance of the venereal disease problem in its social aspects.

Insect-borne Diseases.

Dr. J. S. Purdy (Sydney) discussed the prevention of insect-borne diseases. He considered the subject under three headings, mosquitoes, fleas and flies. He described his experiences in mosquito control and said that it was important to know which variety of mosquito was being attacked. He referred to the difficulty of arousing the sanitary conscience of the community. Under the heading of fleas Dr. Purdy described the measures which had been adopted in past epidemics of plague for their extermination. It was generally conceded that disinfection of premises, as usually carried out, even with formalin, was useless except that the premises required thorough cleaning after the disinfection. He had been considering the advisability of introducing vacuum dust extraction; they were waiting for the evolution and equipment of a small motor outfit. In discussing the extermination of flies Dr. Purdy described their respiratory apparatus and pointed out that the fumes of kerosene did not act mechanically, but as a direct poison.

At the conclusion of his paper, Dr. Purdy showed some lantern slides depicting mosquito breeding places and the work done in Malaya in the control of these. He also showed a cinema film illustrating the life cycle of the mosquito. The lantern slides were reproduced by the courtesy of Dr. Hunter, of Singapore, and the cinema films by the courtesy of Dr. R. Dick, Director-General of Public Health of New South Wales.

Dr. R. C. Everitt Atkinson (Perth) stated that in the southern parts of Western Australia, for example in Perth and its environs, anopheline mosquitoes were rare. Though some six hundred men infected with malaria had come back to Western Australia at the conclusion of the war, no new infection with malaria had occurred. As far as he knew, no malaria had been reported south of Broome. North of Broome, however, there were extensive outbreaks of dengue every year and a good deal of malaria. Recently in the Claremont mental hospital the treatment of general paralysis of the insane by malarial infection had been instituted and local anopheline mosquitoes were being used as vectors of the disease. In the United States of America, a year or two previously, he had seen in negro houses and outhouses great numbers of anopheles which could be captured very easily. He had been informed that during that year there had been one thousand notifications of malaria in that particular small county.

Dr. W. L. Calov (Sydney) said that Dr. Purdy had referred to the control of malaria in Rabaul. The elimination of anopheles' breeding places in the town of Rabaul was a comparatively simple matter as the *Anopheles punctulatus* in his experience did not breed at any great distance from the foreshore and was also very particular in its choice of breeding places. There were, however, a few malarial infections in the town of Rabaul produced by the invasion of anopheline mosquitoes from outside the town boundaries. In regard to the control of dengue fever in Rabaul, this was a much more difficult problem as the *Aedes argenteus* was very difficult to exterminate. In his experience one of the most prolific breeding places for *Aedes argenteus* was the roof gutters of the houses which were often not very efficiently constructed and were apt to sag at various points and to contain water. In addition to this the *Aedes argenteus* appeared to be a very hardy mosquito. Efforts directed towards malaria prevention had in the past been made mainly at the destruction of mosquito breeding places. This was satisfactory where unlimited funds were available, but might be quite impracticable in some poorer countries. The committee of experts commissioned by the League of Nations to report on malaria control in central Europe had recommended in those countries that improvement of hygiene and living conditions generally, the encouragement of agriculture and the free issue of quinine should be instituted rather than that very extensive reclamation and drainage work should be instituted.

Dr. Robert Dick (Sydney) suggested that the most important steps towards the elimination of malaria might be summed up in the one word, bonification, improvements in housing, drainage *et cetera*. No hard and fast rules could be laid down; what was good in one country might be of little or no value in another. The New South Wales Government had been wise enough to realize that methods of destruction of breeding places, such as drainage and reclamation of swamp land, would have the effect of improving the land and greatly enhancing its value. It was gratifying to note that the Government was about to proceed with the work of reclaiming swamp lands in the metropolitan area. The anopholine mosquito was fairly common in many parts of New South Wales and a few cases of endemic malaria had been reported from time to time. It should be noted, however, that other factors besides the presence of anopheles were necessary for the endemicity of malaria. He had been interested in the chairman's statement that anopheline mosquitoes were being used for the therapeutic transference of malaria in Western Australia and he thought that this was the only instance of such a method being used in Australia. In New South Wales the transference of malaria in the treatment of general paralysis of the insane was made direct from patient to patient. In England anopheles had been collected in low lying parts of the country, had been infected with malaria and dispatched to asylums all over the kingdom. As far as he was aware it had never been proved that any of the Australian anopheles were capable

of carrying malaria, though he believed Dr. Heydon had incriminated the New Guinea *Anopheles punctulatus* as a vector. He stressed the simplicity of exterminating the plague carrying flea and compared the expense involved during the 1900 epidemic of plague and the saving brought about by more recent knowledge in the epidemicity of plague. The difference in expenditure was colossal. The methods used in 1900 consisted in the widespread application of whitewash and even such mediaeval methods as the burning of tar barrels in the streets had been suggested. The occupier of premises had been compelled by law to keep his premises free of rat fleas and the cost to the Government had become almost negligible.

DR. G. M. HEYDON (Townsville) said that quite recently in Western Australia anopheline mosquitoes had been used to transfer malaria to mental patients. Some of these anopheles had been examined in the Australian Institute of Tropical Medicine at Townsville by Dr. Baldwin and Mr. Taylor who had found that they were *Anopheles annulipes*. This provided complete proof that this species of anopheles was a possible vector of malaria in Australia. Dr. Heydon said that he had dissected some of these mosquitoes and had examined some after making serial sections, but the results had been inconclusive owing to the unsatisfactory manner in which the insect had been preserved. He had not been able to find the developing parasites in the body of the mosquito. It had been suggested that *Anopheles bancrofti* was an important carrier, perhaps the more important, but no evidence in support of this suggestion had ever been obtained. At the Australian Institute of Tropical Medicine, Townsville, Dr. Baldwin and Mr. Taylor had recently fed some *Anopheles bancrofti* on the blood of a patient suffering from malaria with a view to ultimate dissection in order to discover whether the life cycle of the plasmodium was continued in this species. He was not aware yet of Dr. Baldwin and Mr. Taylor's finding. Malaria in Australia seemed to be diminishing within recent years. In the middle of the nineteenth century the disease had been serious even as far south as Brisbane. For many years it had been absent in Brisbane and in places such as Cairns it had become scarce. This could not be attributed to anti-mosquito measures as very frequently such measures were impossible to carry out in small towns. He suggested that the reason was the same as in parts of Europe, namely, the improvement in the standard of living of the population generally. Quartan malaria was confined largely in the countries in which it occurred to people living in the poorer quarters. In New Guinea it was confined practically to the natives. The standard of living seemed to be more important in regard to quartan malaria than to the other types. He pointed out that Cleland and Bradley had been the first investigators to show that *Aedes argenteus* was a vector of dengue fever. Australia was unique in that the border line of the occurrence of dengue was definitely marked and presumably corresponded to the line where the vector ceased to exist. A sufficiently elaborate survey during an epidemic of dengue and for a year thereafter would demonstrate whether or not some other vector, perhaps some other Aedes, was concerned. A great deal of work had yet to be done in filariasis. The nocturnal variety of filaria was present in Australia and *Culex fatigans* had been shown to be the vector, but it had not been shown that other mosquitoes might not act as vectors also. Brug, of Java, had recently found a new species of filaria which he had called *Filaria malayi* and which was not carried by *Culex fatigans*. Brug had examined some Australian blood smears sent to him from Townsville and had found that none of the microfilariæ belonged to the Malayan species.

Dr. Purdy in reply thanked those present for the interest they had taken in his paper and referred to the work of Brug in Java in praiseworthy terms.

The Prophylaxis of Cancer.

DR. LOCKYER POTTER (Melbourne) stated that he had carried out an experiment in his private practice with apparent success. He sent a formal invitation to a private lecture on malignant disease to a large number of his female patients. Eighty-seven attended the first lecture. Another invitation was sent to a second lecture and sixty women attended. Three further lectures were then given and in the aggregate no less than one hundred and ninety patients attended and listened to what he had to say about cancer of the breast and uterus and were shown many lantern slides illustrating what he had told them. In 1929 he gave a second series of lectures. The number who had attended at the four consecutive lectures, was eighty-eight, ninety-four, one hundred and thirty-nine and ninety-seven. The women had been most attentive and had exhibited great interest in the subject. He said that formal information could be given to women by their family doctors in this way.

Dr. Potter then showed some lantern slides which he stated he had used to illustrate his popular lectures. Included in these was a series of slides on which pithy instructive sentences had been embodied.

DR. R. C. EVERITT ATKINSON (Perth) said he thought that Dr. Potter had evolved a method of getting information to the people who needed it most. A difficulty in the way of this type of propaganda was that many women did not want to know about cancer. They feared it and the very word was anathema to them. He thought that Dr. Potter's work might be a fit example for other prominent men to follow. In the United States of America the importance of an annual medical examination was stressed. By this means many early cases could be picked up.

DR. ROBERT DICK (Sydney) said the work of public instruction concerning cancer had not been entirely neglected by the Public Health Department of New South Wales. As far as possible the subject was brought before the community during health week and posters were continually displayed at railway stations and other places throughout the country. He thought that cancer organizations might do more in the way of confining themselves entirely to research and treatment. At the next meeting he would certainly draw the attention of the cancer committee to Dr. Potter's work.

DR. J. S. PURDY (Sydney) thought that Dr. Potter's paper had been one of the best contributions to the Congress. Health officers were always saying that they should have greater cooperation with the general practitioners. Dr. Potter as a general practitioner had shown how he disseminated cancer propaganda among his own patients. He thought that Dr. Potter's method should prove very effectual and that if it could be made general, great good should arise from it. In South Africa attention had been directed to the increase of cancerphobia among women as a result of propaganda. He thought that this was unlikely to occur if Dr. Potter's method were followed. He would like to see Dr. Potter's lecture printed and used as a standard. He would like to see the ban on public lecturing by doctors lifted in Australia as it had been done in England.

DR. J. COOPER BOOTH (Sydney) thought that propaganda in respect to the advisability of an annual medical examination should come from the insurance companies rather than the medical profession. This was being done in the United States of America.

The Control of Venereal Diseases.

DR. J. COOPER BOOTH (Sydney) set up the thesis in his paper on the control of venereal disease in New South Wales that the solution of the problem would be found not in any new idea, but in the correct blending of the ideas that had already been evolved. He gave an account of the *Venereal Diseases Act* of 1918 which represented the first real attempt to control these diseases. He described the provisions of this measure. The *Prisoner's Detention Act*, 1908, was amended in 1918. The *Vagrancy Act* controlled the actions of prostitutes and of persons trading on them. The *Venereal Diseases Act* had been in force for nine years. Various weaknesses had become evident. Complete success could not be attained unless the whole-hearted cooperation of the medical profession could be secured. There were eight public clinics in Sydney and the boards of every subsidized hospital in

the State were expected to make arrangements for the treatment of persons suffering from venereal disease. The clinics were capable of improvement. Those controlled by the government were the best. Dr. Booth stated that the steps taken to prevent these diseases were practically nil. The interest of the press had not been secured. In consequence the force of public opinion had not been secured. When the public realized the seriousness of the problem, they would be able to overcome the inertia of centuries and begin to eradicate this legacy of the past. Many medical practitioners did not notify the infections in their patients. Large numbers of infected persons sought the aid of pharmacists and plausible quacks. He ventured as a guess to state that the number of new infections in Greater Sydney each year exceeded twenty-five thousand and in Newcastle over two thousand. About one-fifth of those notified as suffering from venereal disease were reported as defaulters and it was ascertained that more than one half of them had given false names and addresses. In dealing with the remedies, Dr. Booth wished to educate the medical profession in regard to the damage inflicted by venereal diseases. He emphasized the point that gonorrhœa was not the simple complaint it was supposed to be and that syphilis was still a great killer. An atmosphere of curiosity regarding syphilis and its complications had been created among medical practitioners; he regarded this as encouraging. He advocated the better training of medical students in the subject and expressed the wish that a lectureship in venereal diseases should be instituted. The medical profession should educate the public in regard to venereal diseases. He held that prophylactic measures should be employed in order to reduce the incidence of infections. Treatment by any person other than a registered medical practitioner should be prohibited. A certificate of freedom from disease before marriage would be required. He thought that this regulation should be a Commonwealth one. He made various suggestions in regard to the coordination of the several clinics. In regard to "red light districts" he stated that they were a menace to any city. The licensing of prostitutes and of brothels had not proved a success in other countries. Dr. Booth found that the question became much more difficult in view of the fact that the amateur was responsible for a great deal of infection. The only way to reach the amateur was by educating the public. The act should be strictly enforced and an endeavour should be made to provide modern facilities for the prophylaxis and treatment of venereal diseases.

Dr. Robert Dick (Sydney) congratulated Dr. Booth on his paper in which the difficulties and failures of the *Venereal Diseases Act* of New South Wales were pointed out and suggestions were made for its improvement. He thought Dr. Booth had mentioned all that public health authorities were capable of doing in the way of improvement in venereal disease control. He had observed conditions in various countries and had come to the conclusion that the state of affairs in regard to venereal disease in Australia was no worse and no better than in such places as the United Kingdom, the United States of America and Germany. In the early part of the war many of the men were infected with venereal disease. It was then not difficult to obtain great sums of money for use in providing for prophylaxis and treatment as the most urgent desire of all the governments concerned was to make soldiers fit for duty in the front line and to keep them so. Since the war, however, there was a tendency on the part of governments to do less and less in the attempt to control venereal disease and to leave the matter more and more to the attention of the local authorities who had to devise methods for combating the venereal disease evil at their own expense. The British Government had paid a regal subsidy to the British Hygiene Council and had utilized its services. Under the new *Local Government Act* it was proposed to do away with this subsidy and leave the subject of venereal disease control to the attention of the local authorities. In the United States of America activities in venereal disease control varied in different cities according to the enthusiasm of the health officers. The best clinic he had seen in any part of the world was in Chicago. The establishment of this clinic had been made possible by the help of wealthy merchants of the city. It was not entirely free; people who could afford to pay, were expected to do so. The lines on which it was run appeared to be very successful. A notable feature in the American clinics was the employment of women clerks who questioned the patients in a mixed clinic quite freely and in a matter of fact manner. This was rather surprising to one who had just come from a British community. Many of the very useful clinics in other parts of the world were housed in quite cheap buildings, but were provided with very full equipment. This was what should be aimed at in New South Wales. At the Rachel Forster Hospital there was a fine clinic for women, but one was badly needed for men.

Dr. K. S. Macarthur Brown (Sydney) wished to mention that the small attendance at the clinics was no index of the importance of the subject. He was interested in the question of sex education, but it should be realized that only a section of the community, perhaps 30%, would benefit. The majority of people could be instructed only by coercion. At the Girls' Industrial School at Parramatta a series of lectures was delivered regularly with the object of giving instruction on elementary biology, reproduction and disease. Reforms were necessary, but could not be brought about suddenly and must go hand in hand with increasing knowledge on the subject. He felt that the community was fortunate in having a man like Dr. Booth at the head of affairs in New South Wales. He agreed with Dr. Booth in his remarks on the necessity for improved teaching for students; he himself had been appalled when he looked back and realized what little knowledge on venereal disease he had possessed on graduation. He agreed that the institution of post-graduate study on venereal disease was highly desirable. To his mind no subject was more interesting from the clinical, bacteriological and sociological aspects. A man without good knowledge of venereal diseases could not be a good practitioner. He believed that the public should be educated, not by a campaign, but in a quiet, unostentatious manner. Much good might come from the display of suitable cinema films. The social aspect was one that merited a good deal of thought. The factory girl of fifteen or sixteen whose home life was perhaps unwholesome, whose wages were poor and whose moral sense was somewhat astray, was apt to be attracted by the comparative ease and luxury associated with life on the streets. Prophylaxis was a matter which required attention. Whatever view was held in regard to the morality of the question, the fact remained that many people would persist in illicit intercourse and they should be protected.

Dr. Mary Hamilton (Sydney) said she saw only women patients and thought it very salutary to get such a wide vision of the subject as that which had been presented by Dr. Cooper Booth. She agreed with Dr. Cooper Booth in regard to statistics and felt that the general practitioner did not realize how many deaths he signed up as due to cardiac failure that were really due to syphilis. She thought that it was important to apply the Wassermann test to pregnant women. Dr. Booth's suggestion regarding the institution of an insurance scheme for sufferers thrown out of employment by venereal disease was a useful one. Many practitioners failed to realize the importance of instituting treatment for wives and children of their patients and of endeavouring to eliminate the source of infection. Just as in the war men had been sent to the front line to avoid infection with venereal disease, healthy occupation should be found for the leisure hours of young people. Dr. Dick's remarks concerning the attitude of women clerks in the clinics in America had interested her, as she felt that the right attitude was to encourage the general public to regard venereal disease like any other disease. She favoured prophylaxis provided its performance was in skilful hands.

Dr. E. S. Morris (Sydney) said that he agreed in the main with Dr. Booth. He hoped that blood tests on pregnant women would come into universal use, but thought that his hopes might be in vain, as few general practitioners took the trouble even to examine the urine. He thought that insurance for patients suffering from venereal disease was an excellent idea, but was afraid it might be abused and that it might lead to the greater licence of the infected women. Some men were sufficiently unscrupulous as to seek infection. The spread of venereal

disease would never be controlled by legislation. Availability of treatment and the encouragement of the sufferer from venereal disease to present himself for treatment would get rid of a few infected individuals, but real progress was possible only by education. The rising generation should be educated in sexual matters and in the dangers associated with promiscuity. If the time should ever come when the man who bought a woman, were held to be as guilty as the woman herself and liable to the same penalties, there would be less promiscuity and less venereal disease. Dr. Booth had said that the medical practitioner was not concerned with morals. As his first duty was as a citizen, he was concerned with morals. The medical man was not doing his duty to his country if he advocated measures which condoned immorality. Every general practitioner knew that the majority of his patients were profoundly ignorant in matters of sex and that they had little idea of the risks entailed by promiscuity. If an examination were made of the mental outlook of one thousand girls of loose morals, not one of them would be found with a decent ideal in the matter of sex. When the sex urge moved such girls, their curiosity was aroused, but was only gratified by people who were little less ignorant than themselves and the knowledge that they obtained was given in a filthy manner. Thus they obtained an utterly wrong conception of sex and biology. Occupation in the leisure time was of the utmost importance in these days of short hours and good pay. The open street led straight to the front door of those wonderful clinics which Dr. Booth had in mind for the future.

Dr. J. Cooper Booth in reply said he wished to express his thanks to Dr. Dick from whom he had obtained a great amount of assistance. He had seen the instructional system used by Dr. Brown in the Girls' Industrial School. It was an excellent scheme and induced the girls to think. The girls were encouraged to ask questions. This was sex education under conditions in which the teacher was able to keep his students under control. The same value would not necessarily be obtained from public lectures, as questions which arose in the minds of the hearers, were liable to remain unanswered for lack of a suitable person to whom they might be put. He had seen no clinic to excel that at the Rachel Forster Hospital. Wassermann tests on pregnant women had been carried out as a routine at Crown Street Women's Hospital, but he believed that they had been discontinued for some reason. In reply to Dr. Morris he said he did not mean that every person infected with venereal disease should receive compensation, but he did think that people who had been turned out of work on account of the operation of a section of the act, should be provided for. The only public education of any real value was in the home, but unfortunately in these days there was very little home life. Before children could be educated, parents must first receive instruction and this could best be done by the family doctor who would have to be educated; they thus returned to the main necessity, instruction of the medical student. He felt that he was not concerned with the moral side of the question, which should be left to others. If he attempted to look after the morals of the community, he would have no time for attention to venereal diseases. If, for example, a married man was guilty of a lapse and indulged in illicit intercourse, his wife and children should be protected and prophylaxis made available to him. If applied within twelve hours, prophylactic measures skilfully used were valuable and desirable. Furthermore, the clinic for prophylaxis would often make possible the detection of early disease in the case of people presenting themselves too late for prophylaxis.

Dr. M. J. Holmes (Commonwealth Department of Health) read a paper in which he discussed the value of legislation in the control of venereal disease. He traced the history of legislative measures dealing with venereal disease in Australia and said that the main principles on which legislation was based, were the treatment of venereal disease patients by medical practitioners only, the obligation of all infected persons to place themselves under treatment by a medical practitioner without delay, the warning of the patient by the medical practitioner of the infectious nature of his illness and of the legal consequences of infecting others, compulsory and impersonal notification by the medical practitioner of all infections and the personal notification of the patient to the State department in the event of non-compliance with the act. There had been considerable improvement in Australia in regard to syphilis. The factors accounting for this included improved treatment, education and legislation. The value of legislative measures was shown by the fact that in one State more than 80% of defaulters were quickly brought under treatment again by enforcement of the compulsory notification sections of the act. Dr. Holmes laid stress on the educative value of legislative measures. He illustrated his remarks throughout by means of tables and graphs. In regard to gonorrhœa the position was not so satisfactory as in regard to syphilis. The number of notification made annually had remained practically stationary over the previous eight years. The clinics for treatment were becoming more popular and the population was increasing and these facts taken in conjunction with the absence of increase in notifications were favourable signs. Dr. Holmes emphasized the fact that individual opinions as to whether the Department of Health was making the fullest use of the information which it obtained, ought not to influence the medical practitioner in carrying out his portion of the obligations. If defaulters were not notified, the department could do nothing. Disappointment as to the measure of success attending the administration of the legislation in some States had been due in no small degree to the insufficiency of the staff employed in the control measures. In the larger States the full time and attention of an experienced and tactful administrative officer were necessary. In any case the active support of the medical profession was essential to the effective administration of legislation.

Dr. E. S. Morris (Sydney) asked why the figures revealed an apparent decline in syphilis in Victoria, Tasmania and Western Australia and not in New South Wales, where similar conditions existed and perhaps better facilities for treatment were available. It was difficult to believe that the figures gave an accurate indication of the state of affairs. In Tasmania he had made an honest attempt to keep practitioners up to the mark in the way of notification, but had eventually been faced with the problem of the defaulter returning to the practitioner and abusing him. Practitioners often honestly desired to adhere to the spirit of the law, but frankly objected to results such as that mentioned.

Dr. Robert Dick (Sydney) said that in a small city such as Perth it might be a fairly simple matter to get good results from legislation, but he thought that in large cities, such as Sydney and Melbourne, compulsory notification was a hopeless procedure.

SECTION OF PATHOLOGY AND BACTERIOLOGY.

Anæmia.

Professor Peter MacCallum (Melbourne) in his address as President of the Section of Pathology and Bacteriology discussed some aspects of anæmia. He first of all referred to the confused terminology of hæmatology. He went on to say that the elucidation of the problem of diseases of the blood was hampered by a failure to realize that blood findings were only an index of underlying processes of disease. In the light of modern work some revision was needed in the standard of, for example, the normal erythrocyte count and the hæmoglobin value. In assessing to what degree the peripheral blood was indicative of the state of the hæmopoietic tissues the blood volume was often neglected. Chlorosis was the classical example of an anæmia due to plethora, but there were many other conditions in which the blood volume was increased and they were not all anæmias. To assume a normal blood volume was unjustifiable and might lead to quite erroneous conclusions regarding the state of the hæmopoietic tissues. In general it might be said that the hæmopoietic tissues produced a particular cell in response to the appropriate stimulus. The resultant peripheral cytological picture would depend on the balance of blood cell formation, blood cell destruction and blood cell mobilization. The hæmopoietic tissues, like other tissues of the body,

possessed potentialities of response greatly in excess of their physiological requirements. Abnormal stimulation might be easily met; later, signs of inadequacy of response began to appear. The destruction of an appreciable bulk of hæmopoietic tissue would cause the limits of response to be reached earlier and, should the tissue be sufficiently reduced, the normal requirements of the body might be sufficient themselves to produce all grades of inadequacy of response. From the point of view of diseases of the blood, and anæmias could not be divorced from these, the monophyletic and polyphyletic theories of blood cell origin had to be remembered. Professor MacCallum thought that the differences of opinion were not irreconcilable. In the established hæmopoietic tissues there was a determinism about the development of the cells which encouraged polyphyletic views, but embryologically and pathologically the point of fixation of destiny was not so clearly defined. He suggested that it was not necessarily a mere presentation of a slow motion picture of the steps of differentiation, but probably also an expression of potentialities realizable only in certain circumstances and perhaps in a few cases represented in phylogeny as a normal feature in some other species. Turning to the subject of pernicious anæmia, Professor MacCallum said that it was usually stated that the cellular condition of the marrow and the appearance of immature or abnormal cells in the peripheral blood were evidence of the haste with which the cells were being turned out, and that they were being produced with a maximum of effort. He thought that it was more reasonable to suppose that it was an indication of a retardation of the process, that more tissue came into action because of the inadequacy of that already functioning to meet the stimulus of blood formation and that the new tissue was itself subject to the same retarding environment. Hastening of the process should result in rapid maturation, the elimination of steps and the taking of short cuts. The concentration of the attention on the erythrocytes in anæmia was apt to divert attention from the leucocytes, but accompanying changes in these cells were to be expected. In pernicious anæmia this point was probably of fundamental importance. In regard to liver feeding, if his view was correct that in pernicious anæmia the bone marrow as a whole was affected, it was not improbable that liver extract might prove to be of some therapeutic value in histologically related diseases.

Professor D. Welsh (Sydney) expressed his appreciation of the address and the suggestions it contained. He remarked that though there was much still to be learned about phagocytosis, he did not agree with Professor MacCallum's views.

Dr. W. J. Penfold (Melbourne) wished to know what method had been used to measure the blood volume.

Professor MacCallum replied that although cells *post mortem* took up large quantities of material, it was impossible to say whether or not this happened in the same way *in vivo*. In reply to Dr. Penfold he said that the vital red method was used for measuring the blood volume in animals, but in human beings the carbon monoxide method was used; this method required workers who had been carefully trained in the technique of the method. A new method for this estimation was needed.

Corpuscular Medication.

Dr. Leon Jona (Melbourne) read a paper in which he discussed corpuscular medication, the method used in the treatment of inoperable cancer by the administration of lead, copper and bismuth. He referred to an article published by him in The Medical Journal of Australia of November 10, 1928, in which he gave an account of 21 patients treated by this method. The metal was adsorbed to the envelope of the red blood corpuscles. Dr. Jona held that if any relief were given to a patient in the last stages of malignant disease, the treatment was justified. Since the first report was made, 16 other patients had been treated. These sixteen had received copper and lead with ox blood corpuscles prepared in the manner described in the previous communication. Seven had died. The condition of one was poor and eight were described as well. Of the latter all but two had been treated in 1929. In practically all those who died, there had been some temporary improvement. He regarded his results as encouraging and thought that some such treatment might be used in combination with other methods.

Dr. W. J. Penfold (Melbourne) wished to know whether the lesions found in the patients had been examined microscopically and whether they had been treated with lead or lead and copper. He wished to see the appendix with notes of the cases and the methods of preparation of the material.

Dr. C. H. Kellaway (Melbourne) wished to hear about the work Dr. Penfold had been doing on the same subject.

Dr. Jona said that two of the growths had been diagnosed by means of the microscope, but the malignant tumour of the stomach had to be diagnosed on clinical signs only. He then read the method of preparation used and showed Dr. Penfold the appendix.

The Treatment of Poliomyelitis.

Dr. Jean Macnamara (Melbourne) read a paper in which she discussed the treatment of acute poliomyelitis by human immune serum. She dealt with a series of infections which had occurred in Victoria during the period 1925 to 1929 and divided the patients into four groups. The first group included those who received serum in the preparalytic stage, the second group comprised those who received serum at a stage of early paralysis with persistent fever, the third group those who received serum later than the first twenty-four hours after the advent of paralysis, and the fourth group those with the ascending or spreading type of infection. She dealt first with the third group and showed that the use of serum was not justified after paralysis had occurred for some time and was apparently stationary and the patient was afebrile. In the fourth group, as a result of the treatment of twenty-two patients, she concluded that the administration of serum was justified, but said that there was no guarantee that the serum would prevent further spread of the condition. This was not surprising in view of the histological appearances of the affected tissue. In dealing with the first group Dr. Macnamara showed that the syndrome of the preparalytic stage could be recognized. She described the symptoms in detail and regarded the spine sign as the most helpful of the manifestations. If the condition of a patient was such that the presence of the preparalytic stage of poliomyelitis was suspected, lumbar puncture was justified. It should, however, be carried out only if provision were made for the immediate examination of the cerebro-spinal fluid at the bedside and if immune serum were available. In the second group nineteen patients had been treated. Sixteen of these had had no further paralysis and eleven had made a complete recovery.

Stored Antipoliomyelitic Serum.

Dr. F. M. Burnet and Dr. Jean Macnamara (Melbourne) presented a paper in which they dealt with the activity of stored antipoliomyelitic serum in experimental poliomyelitis. They had tested the parasiticidal power of antipoliomyelitic serum prepared at the Commonwealth Serum Laboratories and had tried to determine had long it would retain most of its activity. In their experimental work they had confined their attention to the parasiticidal properties of the serum, its power to render an infective filtrate or suspension innocuous when injected intracerebrally. The spinal cord had been removed from the body of a boy who had died of acute poliomyelitis, and had been stored in glycerin. A filtrate of a 5% emulsion of this cord in saline solution had been injected intracerebrally into a *Macacus rhesus*. The animal had developed typical symptoms; it had been killed and its cord stored in glycerin. A filtrate of this cord had been injected into another monkey and unfiltered suspension had been injected into a fourth animal. Typical symptoms had appeared in these animals. It had been impossible to continue passage by filtrates. No definite increase in virulence had been noted with the injection of cord suspensions. The virus invariably produced unmistakable poliomyelitis when it was injected without immune serum. When immune serum was used, protection was afforded. The

results were shown in tables. It was concluded that the parasiticidal properties were retained for at least three years when the ampoules had been stored at ice box temperature. No significant differences were found between the parasiticidal properties of serum prepared from persons recently convalescent and those affected three years previously. It was stated that if administration of serum were limited to patients in the preparalytic and early paralytic stages, adequate supplies of blood for serum were available. The recognition of the syndrome of the preparalytic stage was thus essential for successful serum therapy.

The Antigenic Value of Anatoxin.

DR. C. W. ADEY (Melbourne) gave an account of the methods applied to determine the antigenic value of anatoxin manufactured at the Commonwealth Serum Laboratories. The direct estimation of the smallest quantity that would confer immunity on a guinea-pig, was too crude a method for practical purposes. The methods adopted were the determination of the immunity index, the measurement of the amount of antitoxin in the blood and the Ramon flocculation method. The immunity index was the number of days required to remove the power of a guinea-pig of six weeks to react to the Schick test by the injection of 0·5 cubic centimetre of the toxin. Controls were needed. Dr. Adey held that there was small danger of error due to what was known as a secondary stimulus, that was the immunizing effect of the toxin employed for the Schick test. He illustrated the results obtained in the testing of the various batches of anatoxin and referred to the variation in the response of different animals. In regard to the measurement of the antitoxin content of the blood after immunization with anatoxin, he pointed out that the results were exact, but that the method was extravagant both in regard to time and in regard to laboratory animals. He raised the question whether the antitoxin content of the blood was actually a measurement of the immunity developed. The Ramon flocculation test was a rapid, economical and reliable index of immunity and therefore of antigenic strength. When diphtheria toxin and antitoxin were mixed together in certain proportions, a flocculent precipitate formed. The test was performed by placing a measured quantity of toxin or anatoxin in a series of test tubes. To each test tube was added decreasing quantities of antitoxin. The tube in which flocculation first appeared, was noted. Usually the cloudiness appeared in a few minutes and the precipitate formed soon after. Flocculation appeared first in one tube, then in the tubes on each side and later in the two tubes adjoining. The measurement of antigenic power depended on the number of antitoxic units in the indicating tube and also on the time taken for the flocculation to appear. He had found that unless the anatoxin had a high antigenic power, it would not produce flocculation. There were two hypotheses concerning the flocculation. The first was that the reaction was that of a specific precipitin. The second was that it was a true coagulation of the proteins attached to the toxin and antitoxin. He did not express an opinion as to which hypothesis was the more likely. There was no evidence that the speed of the detoxication process influenced the antigenic power. Information was given concerning the results of the Ramon flocculation test and the immunity index applied to the various samples of anatoxin.

DR. F. G. MORGAN (Melbourne) had been interested in the work. He wished to draw attention to the difference in antigenic response to avidity of toxin and anatoxin. This suggested a different conception of the effect of antigens in producing immunity.

PROFESSOR P. MACCALLUM (Melbourne) wished to know whether any work had been done on the hydrogen ion concentration of the filtrates.

Dr. Adey in reply said that observations had been carried out. The hydrogen ion concentration influenced the development of the toxin in broth. It was unwise to start with a concentration of above pH 7·2, as it would end above 8·3 and this destroyed the toxin as it was produced. To a certain extent it also influenced the Sorenson titration of amino-nitrogen. The anatoxin itself had a hydrogen ion concentration of pH 8·1. When the formalin was added the figure dropped and it was readjusted and allowed to become detoxicated at pH 8·1.

Snake Venoms.

DR. C. H. KELLAWAY (Melbourne) read a paper on the specificity of immunization to snake venoms. He gave short descriptions of experiments carried out by him in this field. Groups of forty guinea-pigs had been immunized for some months with venoms of the tiger snake, the death adder, the copper-head, the black snake and two Indian snakes, the cobra and daboia. It had first of all been necessary to determine the certainly lethal dose of the venoms for guinea-pigs of the weight of those finally tested with venoms other than those to which they had been immunized. Forty animals had been immunized to the venom of the tiger snake (*Notechis scutatus*). Complete protection had been afforded when twelve lethal doses of copper-head venom were injected. Only two animals had survived out of seven after one lethal dose of death adder venom. No protection had been found to one lethal dose of cobra venom. Complete protection had followed after injection of eight lethal doses of the venom of *Oxyuranus maclennani*. Complete protection had followed the injection of 1·6 and no protection the injection of three lethal doses of the venom of *Demansia textilis*. Protection had followed one lethal dose and no protection 1·25 lethal doses of black snake venom. The forty guinea-pigs immunized with the venom of the copper-head, *Denisonia superba*, had been tested. Dr. Kellaway described the results in detail and summed them up by saying that no definite protection had been observed against the venom of the cobra, death adder, brown or black snakes, but that definite protection had been found against tiger snake venom. When an attempt was made to immunize forty guinea-pigs against the venom of the death adder, *Acanthophis antarcticus*, only eight animals had survived. The small number made the results less certain. When ten lethal doses of copper-head venom were given, complete protection had been noted and one animal had died on the twelfth day after twelve lethal doses. With tiger snake venom complete protection had been noted after eight lethal doses, but one animal had died on the seventh day after twelve lethal doses. When guinea-pigs immunized with cobra venom were tested, the only venom against which a certain amount of protection had been obtained was that of the tiger snake. It had not been possible to obtain protection against the venom of copper-head, death adder, tiger snake and cobra when guinea-pigs were immunized with venom of daboia (*Vipera russelli*). Only five animals had survived immunization with the venom of the black snake (*Pseudechis porphyriacus*) and these had not been protected against the venom of the tiger snake given intravenously. Animals immunized against tiger snake venom had been protected when tiger snake venom was given intravenously. All other injections had been given subcutaneously. Dr. Kellaway said that the general results of these experiments emphasized two aspects of immunity to complex bodies like venoms. Moderately close relationship between the species from which the venoms were obtained, permitted of some degree of non-spcific immunity. In the few intravenous experiments carried out this appeared to be more important than the coagulant or non-coagulant nature of the venoms. On the other hand immunity to some components of the venoms seemed to be more easily acquired. It was more difficult to immunize against the venoms which possessed a hæmorrhagin factor than those in which neurotoxins were the important killing agents. Unfortunately the use of detoxicated venoms complicated the interpretation of the experiments, as the action of formalin on proteins had so far been incompletely investigated.

DR. H. O. LETHBRIDGE (Narrandera) desired knowledge suitable for a country practitioner. He had become rather scornful of snake bites after twenty years in practice. He thought that school children should be taught to know more about snake bite and to recognize various snakes, especially the difference between the tiger and the brown snake as this would be very necessary for treatment with the antivenene. He mentioned that the old fashioned method of applying ligatures and leaving them on for a

long time was still in use and sometimes had dire effects. People were still made to walk about and were given quarts of whisky.

Dr. W. J. Penfold (Melbourne) wished to know whether there was a possibility of producing a single serum of universal applicability.

Dr. A. H. Powell (Toogoolawah) mentioned that the black snake did little harm to adults; children were more affected by its bite. He had never seen any bad effects from ligatures. He also wished to know when the antivenene would be available and what method of distribution would be adopted.

Dr. Kellaway in reply said there were many pitfalls in preparing an antivenene potent against more than one snake. There was no protection against the brown snake venom by any of the antivenenes. The brown snake to add to the difficulties produced very little venom. It was their present intention to produce antivenene serum against the tiger snake, copper-head and death adder venom. More work was required to produce a polyvalent antiserum. He thought that ligatures were useful in the early stages of snake bite, but it was necessary to apply them properly around the part of the limb where there was only one bone. It was essential that the ligature should be applied quickly, as the snake venoms were highly diffusible. He also mentioned that the exact knowledge concerning the regions where different varieties of snakes existed was very scanty and this knowledge was necessary for the correct distribution of antiserum.

Dr. F. G. Morgan (Melbourne) read a paper in which he described his work in the preparation of curative sera against the venom of certain Australian snakes. He said that they had first of all used rabbits for the preliminary work of ascertaining the effect of spacing the dosage and varying the route of injection. Owing to the extremely toxic nature of the venom they had commenced immunization with small doses which always consisted of less than one-twentieth of the estimated single fatal dose for that animal per kilogram of body weight. On account of the length of time required for immunization it was obvious that if any method of detoxication of the venom could be discovered to preserve its full antigenic value for immunization against all the important toxic principles of the venom, considerable progress would be made. They had tried the effect of the formalinization of tiger snake and death adder venoms. The toxicity had been tried on guinea-pigs and rabbits. The work was incomplete and the results inconclusive. It had been found, however, that the venom of the death adder was much more resistant to the formalin than tiger snake venom. The action of radium in detoxicating the venoms had been investigated, but this work was also unfinished. It had been found that death adder venom proved much more resistant to the destructive effect of β and γ rays than tiger snake venom. In immunizing horses they had decided to start with a small, single dose given every week and to increase the size of the dose by 50% on each injection, after this the dose had been increased by only 25%. The increase had been carried on until nearly eight fatal doses for the horse had been given in the case of death adder venom or 17 estimated fatal doses of tiger snake venom. Thereafter the increase had been only 10%. Dr. Morgan then explained the procedure that had been adopted in estimating the protective value of serum against the venoms. In regard to tiger snake venom it had been found that roughly one-eighth of a cubic centimetre of serum had protected a guinea-pig of 400 grammes weight against five certainly lethal doses, but that one-twelfth of a cubic centimetre had failed to save the animal. Attempts to neutralize the effects of copper-head and death adder venoms by tiger snake antiserum indicated so far no protective value against death adder, but some slight protection against copper-head venom. In the immunization experiments with death adder venom it had been found that 1·0 cubic centimetre of serum prolonged the life of a guinea-pig to three days when mixed with five fatal doses and that smaller doses of serum failed to save against the same dose of venom. The results were not regarded as very encouraging. The general conclusion was that there was a likelihood of preparing a highly protective serum against tiger snake venom which protected to some extent against copper-head venom. The serum from the horse immunized against death adder venom yielded serum which so far was but slightly protective against death adder venom and which afforded no appreciable degree of protection against either tiger snake or copper-head venoms. The work was still proceeding.

After Dr. Morgan had read his paper, Dr. Kellaway said that he had been very interested in his results with death adder antivenene. It was only in this connexion that Dr. Morgan's results differed from his own. Dr. Kellaway had obtained evidence of some protection against the tiger snake venom as well as against the death adder venom with the same serum, but Dr. Morgan had not.

Professor P. MacCallum (Melbourne) asked whether the composition of venoms was known or whether it had been possible to classify them among the types of chemical poisons. He also asked whether there were any alterations of the venoms with the season, change of diet and other factors.

Endometriomata.

Professor J. Burton Cleland (Adelaide) read a paper in which he discussed endometriomata from the pathological point of view. He said that there were three points which had to be considered: Whether endometriomata constituted a single entity or whether they might arise in two or more independent ways; whether the tumours were true neoplasms or whether they represented merely misplaced tissue or grafts; whether malignant changes arose in the endometrial tissue. If the old theory of misplacement of tissue during the normal development of the Müllerian duct was considered, it was possible to conclude that the growths might or might not be neoplastic. It seemed in the majority of instances that the dislocation was not so complete as to entitle the tumours to the term of true tumours. According to Sampson's theory the growths could be looked upon merely as implantation cysts or grafts and were not really neoplastic. If they were regarded as due to change in the peritoneal lining cells, they were not neoplastic in the true sense, but were heterotopic. Professor Cleland concluded that in whatever way they were considered, they were scarcely true neoplasms. They were merely in some cases neoplastic-like; much of their bulk might be due to retained blood, the building up of new tissue was not often abundant and part of their size was due to surrounding fibrosis. The essential tissue of endometriomata consisted of tubules whose epithelium was similar in all respects to that of uterine mucosa. The tubules were surrounded by stroma similar to that surrounding the tubules in the uterus. These were essential elements in the picture and associated with them there might be unstriped muscle; the muscle usually belonged to the invaded tissue. It was obvious that the tissue of the growths was endometrial. Certain changes might take place. Cysts might form and the stroma might almost disappear. Spaces might form, representing uterine cavities. The ectopic uterine mucosa might burrow in various directions between the muscle layers of the wall of a tissue like the small intestine. There was often much connective tissue associated with the endometrial tissue. Professor Cleland then described the formation of the tarry cysts and said that when leakage occurred from these cysts they might become adherent to neighbouring structures and these might become invaded. In referring to the ætiology of endometriomata, Professor Cleland said that unless some strong evidence were brought forward in the future in its favour, the theory of origin from the Müllerian duct could be dismissed. The origin from the Wolffian body could also be dismissed. Sampson's theory appealed most to him. He thought that the nature and distribution of ectopic endometriomata was such that they all had the same ætiological origin. Heim had succeeded in growing endometrial implants on a mixture of fowl and human serum to which extracts of fowl or human embryo had been added. Fragments of curetted human specimens had been cultivated, but no success had been obtained with endometrial fragments which did not include a segment of the basal layer. Transference from the uterine mucosa would not explain the occurrence of

endometriomata in scars when the uterus had not been interfered with. It was possible that in these circumstances the patient had been menstruating at the time of operation and that some endometrial tissue had been seeded into the wound. Although Nicholson had ably supported the view that these growths arose from local transformation of the peritoneal mesothelium into endometrial tissue, Professor Cleland did not think that the theory was as likely to explain the facts as the view advanced by Sampson. Some endometriomata had been attributed to extension of uterine mucosa through the wall of the uterus into the broad ligament and thence to other situations. Sampson had also emphasized the possibility of secondary implantation from rupture or leaking of tarry cysts and had sought to explain the occurrence of distantly situated endometriomata by invasion of the venous or lymphatic spaces. As far as development of malignant disease in endometriomata was concerned, it seemed quite likely that occasional carcinomata might develope in endometriomata in the ovary. Elsewhere such an occurrence must be very rare. Professor Cleland concluded by giving a short description of the clinical history and microscopical findings in fourteen endometriomata. Eleven of these had been found at the Adelaide Hospital by Dr. L. V. Bull during the routine examination of 2,741 specimens between January 1, 1924, and June 30, 1929. This number of specimens had included 654 from the female generative organs.

DR. E. S. J. KING (Melbourne) read a paper in which he dealt with the subject of endometrioma. He had first been inclined to accept the view that the tissue in the lesions was actual endometrium. Professor MacCallum had suggested that the structures were of local origin. He had investigated a large amount of material. Some of the cysts of the ovary were seen to be undoubted luteal cysts. They had found that outgrowths from these cysts might occur. Glandular appearance was, of course, observed and the gland-like spaces were surrounded by a tissue very similar to, if not morphologically identical with the endometrial stroma. The converse of this observation, the demonstration of cysts showing an endometrial lining and associated glands, had remained to be made. They had found evidence of the identity of the apparently different cysts. Serial sections were often necessary for the demonstration of both luteal tissue and columnar epithelial lining. Gradations might also be found between the atretic follicles and other epithelial-lined cysts. In regard to the occurrence of malignant change in aberrant endometrial tissue, he had reported a case of typical carcinoma of the ovary growing into a large blood cyst. Evidence had been found of the luteal nature of the tissue. The association of multiple luteal cysts with hydatidiform mole suggested that the cause was possibly hormonal and outside the ovary. Dr. King concluded with the remark that all they were doing in regard to the question was guessing. The various guesses might explain part of the complete answer or they might be completely wide of the mark.

DR. J. BERNARD DAWSON (Adelaide) read a paper on the symptoms and treatment of endometriosis. He said that during the past twelve months he had found the condition in only two of two hundred abdomens which he had opened at operation in the Adelaide Hospital. The condition generally occurred in women between the ages of thirty and forty-five. The fecundity of the woman was disturbed. It was difficult to explain this, for the tubes were almost invariably patent, an essential feature if Sampson's view of the ætiology were accepted. The response of the ectopic endometrium to the hormonal stimulus to menstruation should be remembered, for it gave rise to characteristic cysts and resulted in the dense adhesion between viscera. The association with retroversion and retroflexion of the uterus was important on account of the interference with the proper outflow of menstrual discharge and the escape of blood from the abdominal ostia of the tubes. He had previously suggested that ectopic pregnancy might be dependent on the presence of endometriosis. He also thought that possibly the presence of a philogenetic area of endometrial tissue was a precursor of ovarian pregnancy. This was, of course, merely an hypothesis. Dr. Dawson then went on to consider the various symptoms produced by uterine, ovarian, Fallopian tube, general and recto-vaginal septum endometriosis. He showed that the type of pain depended on the situation of the lesion and that the signs produced also varied with the organ involved. Preoperative diagnosis was difficult, but was becoming more frequent as the peculiarly dense and puckering adhesions were recognized. After laparotomy it was usually quite easy to detect chocolate cysts. Endometriosis of the recto-vaginal septum might be confused with carcinoma and frequently an accurate diagnosis could be made only with the aid of the microscope. Treatment depended upon the extent of the pathological lesion. He thought that it was wise to supplement local removal with resection of portions of the Fallopian tubes to prevent further escape into the pelvic cavity of menstrual fragments of endometrium. The main problems to be considered were the necessity and advisability of performing hysterectomy, the use of complete oophorectomy, the use of radium and of X rays. If endometrial invasion could be eliminated by local excision, it was better to leave the uterus, provided its supports were not interfered with. Complete oophorectomy was often necessary, but he thought that this operation might be needed less often when knowledge of the effect of radium had increased. Encouraging reports had been received in regard to tratment by radium. If the results could be reproduced and confirmed, there was available a valuable agent. X rays did not appear to be of much avail.

PROFESSOR D. W. WELSH (Sydney) said that he had listened with great interest to the papers as he had had many specimens of this condition. Whether it should be regarded as a true new growth was debatable. He brought forward the problem as to when hyperplasia became new growth. In the early stages there was an overgrowth, later a new growth and the dividing line was difficult to define. This was seen especially in the prostate and in the penetration of the decidua by the trophoblast. There were varying degrees of penetration of the endometrium into the muscle. There was no difficulty in explaining the islets of endometrium in the uterus, as all had come from the endometrium and maintained their continuity, but at times when subperitoneal adenomyoma formed, the endometrium was pinched off and blood cysts resulted. He had examined growths in the bladder, colon and appendix; he had not had many opportunities to examine the uterus; still the reasonable explanation was that the endometrium came from the uterus. Without assuming the other three possibilities, endometrial tissue arose from the mature endometrium. There was a progressive wandering through the wall of the uterus and then into the viscera. Another point was that the overgrowth and the new growth were due to repeated hormonal stimulus of normal mature tissue. Neoplasms depended on biological and biochemical factors. He would have hesitated to call many of Dr. King's cysts endometriomata.

DR. A. NORMAN MCARTHUR (Melbourne) wished to speak about the treatment. The diagnosis was not usually made until the abdomen was opened. In his patients the cysts had always been bilateral. When the ovaries were free, he removed them as he was nervous of leaving fragments of ovary in case there was more endometrioma there. When there were obvious adhesions from the uterus to Douglas's pouch he removed the whole organ as it was damaged and likely to reproduce vestigial elements. He had found radium given through the vagina into the pelvic tissue very difficult to deal with.

PROFESSOR P. MACCALLUM (Melbourne) said that Dr. King's specimens indicated a luteal origin, but they could be followed to a stage where they were the same as endometriomata. To justify Sampson's views it was necessary to postulate a spread by veins, lymphatics or the Fallopian tubes, with retrograde peristalsis. Professor Welsh's contention of a spread through the uterine wall was well established. It was the normal function of the ovary to shed ova into the abdominal cavity and the frequency with which cysts ruptured and shed their contents into the abdominal cavity, provided many possibilities for implantation and development. Peculiar pathological consequences sometimes developed. The new theory required a power-

ful refutation; it fell into line with Nicholson's work. He had formed no definite opinion of the treatment of these conditions.

Dr. Dawson said that no one could dogmatize about the origin of these tumours. He thought that Dr. King had been talking about a different condition. He found it difficult to explain a luteal cyst in an abdominal scar and the condition in the wall of the sigmoid and colon. He agreed with Dr. King about the necessity of cutting many sections. He did not follow Dr. King's view of the cause of the sterility of the patients with the cysts. He agreed with Professor Welsh that the study of these tumours should throw light on tissue growth. In reply to Dr. McArthur he agreed that preoperative diagnosis was difficult. He had noticed a curious puckering of the wall of the vagina, which was characteristic.

Chocolate cysts were always bilateral; he did not advocate double oophorectomy.

Intracranial Aneurysm.

DR. P. P. LYNCH (Wellington) read a paper on intracranial aneurysms. He confined his remarks to aneurysmal dilatations of the vessels in the circle of Willis and its large branches. He adopted the classification of Fearnsides into mycotic aneurysms, aneurysms due to gross atheroma, aneurysms due to syphilis and aneurysms with no demonstrable inflammatory basis, described as congenital. Mycotic aneurysms were associated with endocarditis and were caused by septic emboli. Aneurysms due to atheroma were passed over by Dr. Lynch and he said that he had not seen an aneurysm due to syphilis. So-called congenital aneurysm occurred in healthy young subjects who were not affected by syphilis, endocarditis or nephritis. There was sufficient evidence to show that congenital aneurysm was one of the commonest causes of subarachnoid hæmorrhage. Dr. Lynch reported in detail nine cases of aneurysm. These had been encountered in the course of routine hospital *post mortem* examination extending over a period of five years. His attention had first been drawn to the condition by the observations of Drennan who regarded the aneurysms as a result of a "plumbing" effect in the vessel wall. Among the cases reported by Dr. Lynch was that of a girl, aged sixteen years, who had recovered from a typical subarachnoid hæmorrhage and in whom no evidence of endocarditis, renal disease or syphilis could be found. He pointed out that there was as a rule a definite train of symptoms. There was a sudden onset of headache with stiffness of the neck and changes in temperament leading to a diagnosis of hysteria and vague eye troubles varying from pains in the eyes to ocular palsies and actual blindness. The most striking symptom was due to involvement of the oculo-motor nerve. From the ætiological point of view the use of the word congenital implied aneurysm formation without antecedent damage to the wall of the vessel and they were forced to the conclusion that the underlying cause must be a structural congenital defect in the vessel. Before a conclusion was formed in any particular case, it was necessary rigidly to exclude syphilis and other diseases which gave rise to aneurysms either directly or indirectly. Aneurysms of this kind had been known to occur with other structural defects in the vascular system. There was a great variation in the vessels of the circle of Willis. These vessels had been investigated by Busse who had found that in only 42% of bodies did the arrangements correspond to the usual anatomical descriptions of the text books. Busse had found that in no fewer than 10% there was a local dilatation of the vessel with the formation of a sac sufficiently large to deserve the name aneurysm and he had also found on microscopical examination of the thin walls of these sacs that the *tunica media* was rather deficient or absent. Rose Bradford had pointed out that these vessels were characterized by the absence of much fibrous tissue. They were also peculiar in their mode of support and the larger vessels came off in all possible planes. The absence of any considerable *adventitia* threw all the responsibility for the integrity of the vessel wall on to the *tunica media*.

DR. W. KEITH INGLIS (Sydney) expressed his appreciation of the paper, as congenital malformations had a special attraction for him. It was encouraging to him to hear that Dr. Lynch had formed the opinion that these malformations at the base of the brain were congenital. He had found that these congenital conditions were not limited to one part of the body, but others occurred in the same person. He tried to correlate conditions which on the surface appeared quite different and would like to know whether any other malformations were found in the patients. He mentioned the work of Sargent and Greenfield on this subject.

DR. E. S. P. KING (Melbourne) said that he had seen nine patients with this condition in six years. He had been interested to find that in every instance save two there had been some cause indicating a rise of blood pressure at the time of the rupture, such as lifting weights, dancing and once coitus. Two of his patients had recovered from the attack. One a few days before the seizure had had a slight head injury. This was important from a medico-legal point of view.

DR. F. S. HANSMAN (Sydney) said he would like to ask whether Dr. Lynch thought that lumbar puncture was indicated and how much fluid should be withdrawn when it was found to be blood stained.

Dr. Lynch in reply said that he had noticed in the literature cases described of aneurysm of the vessels at the base of the brain in conjunction with coarctation of the aorta and other abnormalities. In reply to Dr. King he stated the effort, strain or injury might easily be the determining factor in causing rupture. In reply to Dr. Hansman he could not say whether or no it was wise to carry out lumbar puncture, but this treatment certainly relieved the headache.

The Pathology of the Corpus Luteum.

DR. E. S. J. KING (Melbourne) read a paper in which he discussed some aspects of the pathology of the *corpus luteum*. He described briefly the life history of the Graafian follicle. He showed that the developing ovum might follow two paths and that either the *corpus luteum* and the consequent *corpus albicans* might result or that the *corpus atreticum* might result. This usually collapsed, forming the *corpus candicans*, or the process might be altered with the formation of the *corpus fibrosum*. Cysts might form from either the *corpus luteum* or the atretic Graafian follicle without a definite *corpus luteum* being formed. The multiple luteal cysts associated with hydatidiform moles were a good example of the latter derivation. Dr. King referred to Fraenkel's classification of luteal cysts into those lined by luteal cells, those in which there was connective tissue internal to these cells, and those with an epithelial lining. In the last variety most of the luteal cells might disappear and it might be difficult to decide the origin of the cysts. He was convinced that Sampson's perforating blood cysts were endometrial only in that they morphologically resembled endometrium. Luteal cysts might rupture into the peritoneum and produce secondary nodules in the peritoneum. The secondary implantations from the so-called endometrial cysts did not prove that they were peculiar formations of definitely extraovarian origin. Carcinoma might grow from luteal cysts.

PROFESSOR P. MACCALLUM (Melbourne) said that some aspects of this paper had been discussed that morning; it was open again for further debate.

PROFESSOR E. F. D'ATH (Dunedin) doubted whether some of the cells described by Dr. King as luteal were so. They appeared to him to be lipoid.

DR. W. KEITH INGLIS (Sydney) said that he felt he had to join the majority in criticising Dr. King's interpretation of some of the slides. Histological evidence could be very misleading and many cells might resemble luteal cells as in hypernephroma. There was difficulty in excluding lipoid cells with phagocytic qualities. He had examined a tumour of the thyreoid gland which looked like a *corpus luteum* because of the lipoid containing cells. He suggested Dr. King should take these criticisms into account when reexamining his material. The question of ectopic gestation was interesting, as it has been suggested that it occurred in misplaced uterine tissue. He had

examined two and found only decidual cells and no uterine glands. When endometrial cells were elsewhere than in the ovary, a wrong diagnosis was not likely to be made, but when they were placed in the ovary the possibility of error was considerable. He was often at a loss to know how to account for these blood cysts.

Dr. King in reply to Professor D'Ath said that it would be difficult to prove that the cells were luteal; the fact that they might be lipoid has occurred to him. He had considered that they might be lipoid phagocytic cells, as the cells were common. He did not think it likely that lipoid cells would be displaced by hyaline tissue. He also had found only decidual cells and no glandular tissue in ectopic gestations.

Cancer in Mice.

DR. T. CHERRY (Melbourne) read a paper on experimental cancer in mice. There was no discussion.

Allergic Problems.

DR. IVAN MAXWELL (Melbourne) discussed allergic problems arising from a review of one thousand persons who were the subject of hypersensitiveness. Of this number 730 had yielded cutaneous reactions, 481 had reacted to grass pollen, 51 to Cape weed, 115 to Cape weed and grass pollen and 83 to other irritants. Patients numbering 465 had suffered from hay fever. All these had yielded cutaneous reactions to pollen; 324 had been treated, of these 276 were completely or nearly completely relieved and 48 were slightly relieved or not relieved at all. In regard to the latter group the attempt at desensitization might not have been sufficiently specific, the treatment might not have been sufficiently prolonged or given at the right time or some associated pathological disturbance might have interfered with the patient's antibody formation. The number of patients suffering from asthma had been 505. Of these 252 had yielded cutaneous reactions and 221 had been treated; 165 were completely or almost completely relieved and 56 slightly relieved or not relieved at all. Hay fever had preceded asthma in 105 instances. As far as infection of the nasal sinuses was concerned, 100 of the 1,000 patients had suffered from sinus infection. Of these 76 suffered from asthma, 20 from hay fever and four from paroxysmal rhinorrhœa. In discussing house dust Dr. Maxwell said that the actual component responsible had not been isolated. In view of Van Leewin's work he had tested patients with the fungus *Aspergillus fumigatus*, but had obtained no reactions. He noted the fact that of 88 patients submitted to fractitional test meal examination, 29 had exhibited achlorhydria and that this had occurred chiefly in those who did not react to cutaneous tests.

DR. E. H. MOLESWORTH (Sydney) read a paper on the subject of sensitization. He said that it was his desire to widen the scope of the discussion and to introduce aspects of sensitization other than those of hay fever and asthma. He referred first of all to anaphylaxis proper. He described the long-lived if not permanent nature of the phenomena and the ways in which it might be diminished or even abolished, and referred to the views of Lewis that it was caused by the release of histamine-like substance. He thought that Lewis's views required reconciliation with the experiment of Dale and Kennaway in which a perfused uterus, after having exhausted its responsiveness to the serum to which it had been rendered anaphylactic, responded just as fiercely as ever to a minute amount of histamine introduced into the Ringer's solution. In regard to sensitization as found in human pathological manifestations, it was necessary to beg pathologists or experimenters upon anaphylactic animals not to be too insistent upon applying the laws of major anaphylaxis to the manifestations of sensitization generally. Although it was possible that a single exhibition of the antigen might induce anaphylactic sensitization in man, it had to be admitted that allergic sensitization in man generally resulted from frequent small repetitions of the action of the antigen. In an animal such an experiment would lead to immunity rather than sensitization. While major anaphylaxis could be induced only by injection, allergic phenomena in man seemed to be brought about by contact of the antigen with the skin or mucous membrane, as in eczema or asthma, or by absorption from the gastro-intestinal tract in urticaria, purpura and eczema. Further, allergic sensitization, at least in urticaria and eczema, but probably also in asthma, might be induced by an antigen which was far lower in the chemical scale than a protein. The suggestion was that these simple substances formed a chemical combination with tissue proteins and then acted as antigens. Dr. Molesworth felt that this was rather straining to make allergy obey the laws of major anaphylaxis. As there were so many differences between the two forms of sensitization, it seemed to him quite logical to admit the difference in chemical structure of the antigens. Desensitization could be brought about in an anaphylactic animal by the single injection of a minute amount of the antigen, but in the case of allergy a long and carefully graduated course of the antigen had to be administered before tolerance was reestablished. The methods of desensitization successful in anaphylaxis might only aggravate allergic sensitization. Attempts to produce non-specific desensitization were much more likely to give good results in allergy than in true anaphylaxis. Again, there was a tendency of allergic sensitization which was at first specific, to become diffuse, so that the reaction could be called forth by the exhibition of dissimilar chemical agents. However, in spite of these differences he could not help being convinced that these two phenomena, as expressed in urticaria, asthma, purpura and many forms of eczema, were closely related. The seat of the reagin in the cell was well demonstrated by experiments of Bloch. That there existed in the serum of allergic patients a substance which could be regarded as equivalent to an antibody was shown by the experiments of Prausnitz and Küstner. Other experiments of Jadassohn tended to show that there was some component of some of the protein antigens that was dialysable. Such experiments kept open the question as to whether the allergic phenomena might be the result of the influence of substances which were not protein in nature. Dale had lately admitted that the histamine-like substance described by Lewis played a part in the production of the phenomena, but Dr. Molesworth was not clear whether they were to assume that the liberation of this substance was responsible for all the varied manifestations of anaphylaxis and allergy. In regard to idiosyncrasy, it was difficult to believe that it was always the manifestation of cryptogenic sensitization. He felt that it was a mistake to try to force idiosyncrasy into the mould of acquired sensitiveness and thought that it was quite logical to hold that true idiosyncrasy might occur, that it obeyed many of the laws of allergy and anaphylaxis, but might arise from causes other than those of these two phenomena. He concluded that anaphylaxis, allergy and idiosyncrasy were allied phenomena. The application of knowledge on these subjects would be valuable not only in hay fever, asthma and urticaria, but in eczema. The allergic origin of eczema was not realized.

Dr. Molesworth showed a patient who had had giant urticaria; unfortunately the lesions had faded.

DR. F. S. HANSMAN (Sydney) presented a child with very resistant eczema who had improved when wheat, milk and eggs had been omitted from her diet. He mentioned that he had treated two patients whose symptoms had cleared up after salt had been omitted from the diet.

DR. C. H. KELLAWAY (Melbourne) said that he was interested from the standpoint of major anaphylaxis. They should be careful in saying that all these phenomena had the same cause. The phenomena were difficult to explain on the views of Dale and Coke. The filtration experiments referred to by Dr. Molesworth were as full of holes as the filters themselves and caution should be exercised when inferences were being drawn from those experiments. He supported Dr. Molesworth about the *H* substance. There was no evidence that it was histamine. They should keep open minds about these phenomena.

DR. E. H. WILLIAMS (Dunedin) asked some questions concerning eczema in babies. They often drifted into the hands of unorthodox practitioners. The lesions had various causes, but he thought it unwise to say that they were all due to protein. There were other causes such as external irritants and dietetic factors, either fat, protein or carbo-

hydrate. He had found treatment on dietetic lines the most successful. He had attended a baby of six months, breast fed for an eczema which had cleared up with local applications. The mother had come back when weaning had been thought to be advisable. The result of one bottle of cow's milk had been a true manifestation of allergy with urticaria, wheezing, vomiting and so forth. He tried desensitizing by means of small doses, starting with half a teaspoonful of cow's milk and gradually increasing, but when the dose reached a teaspoonful the child had nearly died. He had then desensitized it with peptone. At six years of age the child developed asthmatic tendencies.

DR. B. MILNE SUTHERLAND (Melbourne) emphasized the importance of sinus infections in paroxysmal rhinorrhœa. Many patients were definitely sensitive and where the sensitizing substance could be avoided, the results were good. He gave details of an experiment with nickel sulphate which he said was most interesting being an example of a single substance acting as an irritant. Familial tendency was often seen; members of the same family were sensitive to different substances. In twins frequently both had urticaria or hay fever and yielded the same skin reactions.

One member said that he could eat chocolate in Australia but he was unable to do this in New Zealand without bad effects.

DR. J. I. C. COSH (Sydney) speaking of urticaria stated that his daughters suffered from it and their susceptibility varied. It might have been brought on by an undue liking for chocolates. Mechanical factors played a part in producing it, such as pinching. One of his patients had been sensitive to bananas. He discussed the risks attending the injection of antitoxin in asthmatic persons. He and Dr. Molesworth had been interested in a patient with asthma who had worked in a linseed meal factory and had developed eczema. He (Cosh) treated him with dilute nitrate of mercury, lead lotion and *liquor detergens*. He had derived benefit from the treatment. He was interested to hear that purpura might be an anaphylactic phenomenon and described a case he had lately seen.

DR. W. J. PENFOLD (Melbourne) described the condition of an X ray worker in the gelatin room; it might have been sensitization. Mites were also present. He mentioned that Blair Bell believed that he could get anaphylaxis from gelatin in his colloids. He would not expect any result from giving antitoxin in these cases, as the serum was so much altered and concentrated by heat. Unconcentrated serum had had a bad effect in some persons.

In reply Dr. Maxwell said that he purposely had not entered into a discussion of the theory of these conditions but there were a few points he would like to mention. Animals could be sensitized by injection or inhalation as shown by recent work. Passive transfer to the skin took place in 85%. Desensitization took a long time. In regard to Dr. Hansman's patient he would be interested to see the results of the skin tests. He discussed some of the recent work done in America on this subject. He did not think that gelatin was antigenic and expressed the opinion that some other factor must be present. Patients with angio-neurotic œdema at times did not react to the cutaneous tests. The patients might be temporarily desensitized by ingesting the offending substance, they should not be tested after an attack. One of his patients had failed to react for two weeks after an attack. He had had no experience of modifying the antigenic substances with formalin.

Dr. Molesworth said that in these allergic phenomena the greatest difficulty was to locate the antigen. The term angio-neurotic œdema was misleading. The treatment was most successful when the offending antigen was located; this was sometimes very difficult and tedious. In reply to Dr. Cosh he suggested that his daughters had not suffered from urticaria but from dermographia. He was interested in the statement of the member from New Zealand. It tallied with the history of children who suffered from urticaria at the seaside and not in the country. Infantile eczemas, not seborrhœic, were often due to mechanical or chemical factors and responded well to treatment. Some of the patients, however, did not do well and they were as a rule hypersensitive to foodstuffs; they were very difficult to treat.

Criteria of Cure in Gonorrhœa.

DR. C. H. SHEARMAN (Sydney) read a paper in which he discussed the laboratory criteria of cure in gonorrhœa. He said that while laboratory methods yielded much valuable information, there was no single test which could be accepted as an absolute criterion of cure. They should be regarded as a necessary supplement to clinical examination and the onus of declaring a patient free from infection should not rest on the pathologist. Reliance on microscopical examination alone was misleading. He discussed laboratory methods under the headings of microscopical tests, cultural tests and serological tests. In his experience the cultural test had proved one of the most useful and reliable of the laboratory tests. The fact that gonococci had been cultured when pus cells were practically absent, and that some patients with infection of the posterior part of the urethra had yielded no reaction to the serological test, raised the question of whether a tolerance might not sometimes be acquired by the urethral tissues. If this were correct, carriers of the disease would exist in gonorrhœa as in other infective diseases. When tissue reaction was not much in evidence, culture tests offered the best means of detecting the organism. A reaction to the complement fixation test should never be accepted as the sole indication that a patient was non-infectious and in a safe condition to marry.

DR. NORMAN GIBSON (Sydney) read a paper on the clinical interpretation of tests for cure of gonorrhœa in the male. He discussed the various tests and concluded that when there was no discharge at the meatus, when the urine was clear and free from shreds, when the prostate gland, Cowper's glands and the seminal vesicles were normal to palpation and no pus could be demonstrated after massage from these glands when the mucosa of the entire urethra, examined by the urethroscope, appeared normal and when the complement fixation test yielded no reaction, it was extremely unlikely that the patient was not cured. Next to the clinical tests the complement fixation test in his experience had proved itself to be the most important criterion of cure. After the patient had survived all these tests of cure at the end of a known gonococcal infection, he should be instructed to abstain from alcohol and to withhold from sexual excitement for a period of three months. After this period of time he might be permitted to live his routine life, with the exception that if sexual intercourse were resorted to, he should use a condom for a further period of three months. In regard to fitness for marriage the patient should be advised not to marry for at least six months after he had been pronounced cured and preferably not for a period of twelve months.

Gonorrhœa.

DR. R. V. STORER (Sydney) read a paper in which he discussed the practical application of immunity processes to the cure of gonococcal infections. He said that the gonococcus did not produce an extracellular toxin and that therefore it could not produce any inflammatory reaction during its period of viability. The clinical manifestations of gonorrhœa were due not to infection by gonococcoci, but to their death. Gonococcal death involved the liberation of a lytic principle which dissolved other gonococci, the gonophage. The gonophage was probably a true immunity substance and it was possible that its accumulation in the body of the individual accounted for his recovery. It was important to distinguish between true mucosal infection and tissue response to gonotoxin. Purulent discharge resulted only when the gonococci had penetrated into the submucous layers and had liberated their toxin. The body dealt with the infection by three defensive processes, exudation, phagocytosis and antibody formation. Dr. Storer discussed each of these in turn and said that the processes which should be encouraged were death of the gonococcus with liberation of its endotoxin and gonophage, absorption of these two substances into the blood stream and phagocytosis.

DR. P. FIASCHI (Sydney) said that after having concentrated his attention on gonorrhœa for thirty-one years, he had formed certain conclusions. He emphasized what Dr. Shearman had said about microscopical examinations. He looked on gonorrhœa as glandular or non-glandular. Chronicity was limited to infections of the glands of Littré and Morgagni. In regard to the silver nitrate test he said that the human urethra could stand a great deal and he looked upon the provocative test with silver nitrate as being the most reliable. He had used it four times in fourteen days. First he used a 1% solution and instilled it into the urethra. If there was no discharge he used a 2% and if still nothing occurred a 3% solution and examined after seventy-two hours. If there was no definite reaction he injected a 5% solution into the anterior part of the urethra and if no reaction was noted, he felt justified in allowing the patient to marry. In regard to the complement fixation test he had subjected many of his patients to his test and also had complement fixation tests done; nearly all yielded reactions. He quoted many cases. He believed that every gonorrhœa could be cured. Staphylococcal infections were much worse.

DR. A. B. LILLEY (Sydney) asked Dr. Shearman about the method of preparation of his antigen and the length of time it retained its activity. He also wished to know whether he placed as much reliance on a reaction to the fixation test in gonorrhœa as on a reaction to the Wassermann test. He had found that many patients who were not infected with both gonorrhœa and syphilis, reacted to the Wassermann and the gonococcal complement fixation tests.

DR. F. S. HANSMAN (Sydney) mentioned one cause of persistent gonorrhœa, namely nephritis. He would like to ask Dr. Gibson who had had such good results, what his antigen was.

DR. A. J. FITZGERALD (Sydney) asked Dr. Shearman whether he had used Solis Cohen's method in patients with gonorrhœa and whether early removal of serum from the clot affected the Wassermann reaction as it affected the gonococcal complement fixation test.

PROFESSOR P. MACCALLUM (Melbourne) said that laboratory criteria of cure were only relative and all tests should always be interpreted by the clinician. Laboratory tests were only adjuncts. The difficulty in complement fixation was that of securing a satisfactory antigen. He did not regard positive results as being safe guides. The cultural tests depended largely on the method and the media used. The success of the bacteriologist depended on his technique. He never relied on it when he failed to obtain growths. It was necessary to take all facts into consideration and the clinician had the final control.

Dr. Shearman in reply to Dr. Fiaschi said that infections that cleared up easily without treatment, were probably non-specific urethritis; there may have been a profuse discharge, but no specific reactions to the tests were obtained. There was a longer incubation period and they cleared up easily. He had noticed in persons referred to him because of persisting urethritis that caustic had been used. He described the method of preparation of the antigen. The gonococcus was grown first on chocolate agar and then on ascitic agar. The stock was used for about three months. He had paid no attention to the time during which it remained active. He had found no cross-fixation like Dr. Lilley. In reply to Dr. Fitzgerald he had used the Solis Cohen method once, but he had failed to obtain a reaction to any of the tests. The time the serum remained on the clot had no effect on the Wassermann reaction as this was not a reaction due to an antibody like the complement fixation test in gonorrhœa. He agreed that all these tests were only supplementary.

Dr. Gibson emphasized the importance of the clinician and the pathologist working side by side. His experience had been that a reaction to the complement fixation test meant existing disease. Silver nitrate could only affect the urethral mucosa; gonococci might still be present in the seminal vesicles. He had seen patients who had been pronounced cured, returning with iritis and arthritis. He did not believe that a patient who still yielded a reaction to the fixation test, was cured.

Kline and Young's Modification of the Kahn Test.

DR. BEATRIX DURIE (Newcastle) read a paper in which she discussed the modification of Kline and Young of Kahn's flocculation test for syphilis. She referred to an article published on the subject by Dr. T. Hamilton in THE MEDICAL JOURNAL OF AUSTRALIA of November 17, 1929. The advantages of the Kline antigen emulsion over Kahn antigen dilutions were that the former was more stable, might be used immediately after preparation and was more satisfactory at low room temperatures as well as at ordinary temperatures. After describing the method of carrying out the test, Dr. Durie said that 429 specimens of serum had been tested by the Kline method and by the Wassermann test; in 399 there had been agreement, in 10 relative agreement and in 20 disagreement. For these tests the Wassermann test had been performed by a modified ice-box method. In 291 succeeding tests the eighteen hours ice-box method had been used with the Wassermann test; in 279 there had been agreement, in six relative agreement and in six disagreement. An analysis of the 26 cases in which disagreement had occurred, made it appear that the Kline test would prove to be more sensitive than the Wassermann test in very early syphilis and in patients with old infections which had been treated. She thought that at the present time the Kline test should be confirmed by the Wassermann test.

SECTION OF ORTHOPÆDICS.

Stabilization of the Foot.

DR. A. V. MEEHAN (Brisbane) as President of the Section of Orthopædics discussed the question of stabilization of the foot. He gave a *résumé* of the relative values of the operative procedures. For practical purposes all the lateral movement and with it all the lateral deformity in the paralytic foot took place distal to the talus and the highest percentage of lateral movement occurred at the subtaloid and the calcaneo-cuboid joints. It was against these joints that their attack had to be directed. Dr. Meehan divided unstable feet into three groups. The first was that of the flail foot, in which no fixed deformity was present, and in which there was no muscular control over the foot. The second group was that of simple foot drop without fixed deformity. The third group comprised the deformed foot in which lateral or antero-posterior fixed deformity was present. In regard to flail foot Dr. Meehan was still satisfied with the results obtained from the operation of Whitman. Although the operation was mutilating, it had to be remembered that it was performed on account of an absence of stability in a structure which should be stable. The operation reduced the length of the limb and produced an adduction deformity of the fore part of the foot. Dr. Meehan showed how these disadvantages could be overcome and drew attention to the fact that by this operation more backward displacement of the foot could be secured and that the patient could be treated at a much earlier age than by any other operation. The correction of simple foot drop had given rise to a great deal of discussion. At first he had used fixation of the tendons of the *tibialis anticus* and *peroneus brevis* to the tibia. For the last three years, however, he had used the method described by Willis Campbell with good results. He recommended it as an operation which promised to give a permanent result. In regard to the lateral fixed deformities of the foot, treatment should be directed to the obliteration of all lateral movement of the foot by the production of ankylosis of the subtaloid and calcaneo-cuboid joints. At the same time bones should be resected in such a way that as much as possible of the existing deformity would be corrected. Dr. Meehan described various types of deformity and discussed the operative treatment of each.

DR. D. J. GLISSAN (Sydney) agreed with the statement that they should not pin their faith on any one particular operation in the treatment of deformities of the foot. His experience coincided with that of the President, with the exception that he considered a centre of irritation behind the ankle joint might occur after Campbell's operation of bone block had been performed. He observed the same

indications for Whitman's operation as those described by the author, but depended usually on Naughton Dunn's operation, which had been devised to deal with the problems associated with the flail foot. He expressed the opinion that it was necessary to consider the limb as a whole, as some patients with flail foot walked perfectly with the aid of the *gluteus maximus*.. He asked for information regarding the age limits between which the operation should be performed.

Dr. W. Kent Hughes (Melbourne) expressed the opinion that it was easy to push the *os calcis* back into its original position in cases of *talipes calcaneus* by making an incision on the medial side of the foot, stripping the fore part of the *os calcis* and dividing the ligaments attached to it. An advantage of this procedure was that the *tendo Achillis* was not often fully paralysed. He considered that this method or an alternative one of forcing the heel backwards by means of plaster, to be well worth trying before Whitman's operation or Naughton Dunn's operation was tried. He had not done a tendon transplantation for fifteen years, as he considered that it removed the influence of a useful muscle; he hoped some day to see less mutilating operations on bones of the foot. In the treatment of drop foot he had not found it necessary to do more than divide both superficial and deep plantar attachments of fascia to the tarsal joints.

Dr. L. G. Teece (Sydney) endorsed Dr. Meehan's opinion of the value of Whitman's operation. He considered that the bad results following this operation were nearly always due to insufficient posterior displacement of the foot. The posterior aspect of the tibia and fibula should be freely stripped to secure adequate posterior displacement. He advised as a routine a lateral radioscopical control to check the correct condition after operation. Very few flail feet existed in which there was an entire absence of activity in the muscles concerned. He agreed that astragalectomy in adults rarely had a good result. He had had no experience of Campbell's bone block operation, but would try it, in view of Dr. Meehan's remarks. Drop foot was one of the greatest problems in paralytic deformities of the feet, as many people would not wear a toe raising spring.

Dr. C. B. Littlejohn (Melbourne) expressed his gratitude to Dr. Meehan for having expressed the current ideas on the treatment in deformities of the foot in so clear a manner. He agreed that astragalectomy should be done only in children, as he had also experienced bad results in adults. He emphasized the necessity of stripping up the tissues at the back of the tibia; he had not noticed this mentioned in any published descriptions of the operation. His results with Campbell's bone block operation had not been good, owing to persistence of pain.

Dr. Shedden Davis (Sydney) expressed the view that more attention should be paid to a functional result and less attention to a cosmetic result in these operations. Appearances should not always weigh as much as the ultimate function following the operation.

Dr. H. Gilbert (Adelaide) had noticed an extraordinary tendency for a limb to take on active growth after some of these operations for paralytic deformities of the feet. In paralytic *talipes equinus* with contracture of the *tendo Achillis*, in which the anterior tibial group was damaged and the calf muscles had slightly recovered, he considered the nutritional value of the *tendo Achillis* was not equal to that of normal tendon, consequently he always performed an L-shaped lengthening rather than a simple tenotomy.

Dr. L. O. Betts (Adelaide) congratulated Dr. Meehan on his paper. He confessed to the same doubts regarding the efficacy of Campbell's operation as had been expressed by previous speakers. He mentioned Lambrinudi's operation of subastragaloid arthrodesis with the forefoot cocked up and the back of the astragalus forming a natural bone block.

Dr. C. Gordon Shaw (Melbourne) pointed out that recovery of the flexors frequently followed stretching of the *tendo Achillis* in *talipes equinus* deformity and described an appliance for this purpose. It consisted of a cross piece projecting backwards from the usual upright leg iron to which was attached a vertical piston, sliding in a tube attached to the heel behind the attachment of a leg iron which contained a spiral spring. He expressed the opinion that this was the most useful form of apparatus he had seen for this purpose.

Dr. Meehan in reply stated that he had carefully analysed Campbell's results while visiting his clinic and could find only one or two patients in whom pain had persisted for more than a few months. The small fragments of bone fused and made a good graft. Campbell had assured him that bursæ did not form from pressure on the block of bone. He had performed the operation on one adult of eighteen years who was, however, very light in weight; the majority of his patients had been children. In reply to Dr. Gilbert he stated that he always did an open lengthening of a *tendo Achillis* by making several notches on the sides and then sliding the tendon.

Demonstrations of Apparatus.

Dr. Jean Macnamara demonstrated various splints used in the treatment of poliomyelitis in children. For the treatment of paralysis of hand muscles she utilized splints made from paper pulp to which a strong starch solution had been added. These were moulded to the hand of the child and then cooked in a slow oven. It might be necessary to make fresh splints every few weeks as the indications for treatment altered as the muscle balance was disturbed, owing to increasing recovery in one or other muscle. She emphasized the capacity of paralysed muscles to recover even some years after the onset of the disease, provided they were placed in the position of rest. All subjects of poliomyelitis were placed in a modification of the double Thomas's splint in order to avoid the later onset of scoliosis or other deformities associated with paralysis of the back muscles. This modified splint was supported above a frame by movable supports and allowance was made for abduction or adduction of the thighs and a malleable armpiece allowed for similar movements at the shoulder joint. It was important to remember that the balance between groups of muscles altered from month to month during recovery and it was also important to remember Lovat's principle that the patient should be kept in splints as long as any sign of recovery continued.

Dr. W. Kent Hughes (Melbourne) endorsed the remarks of Dr. Jean Macnamara on the capacity of muscles to recover after long periods if placed in the position of rest. He differed from her regarding the optimum position of the arm in paralysis of the deltoid. The arm should not be fully abducted. He considered that involvement of the spinal muscles invariably accompanied paralysis of limb muscles and therefore the patient should receive adequate support to the spine, even though a limb was the only obvious site of paralysis. He emphasized the value of serum in preparalytic conditions and suggested that its distribution and the education of rural practitioners in its use should be undertaken by the Federal health authorities.

Dr. E. G. S. K. Vance (Melbourne) suggested that the prone position should be utilized in the early treatment of poliomyelitis. He used a modified Bradford frame and allowed the foot to fall through a hole in the canvas. The abdominal muscles, the spinal muscles, the leg muscles including the glutei were all well balanced in a position of adequate rest. The arms were attached to malleable splints which could be modified to suit the individual needs.

Dr. Macnamara in reply said that she had used only the prone position for patients in whom the *serratus anterior* was involved as well as the deltoid, as she considered that this was the only satisfactory position. She quite agreed with Dr. Vance that children were much happier in the prone position.

Dr. Jean Macnamara demonstrated dissected specimens and photographs illustrating an operation devised by Dr. Lockyer Potter for the treatment of residual paralysis following lesions of the ulnar nerve. The principle involved was to secure stability of the metacarpo-phalangeal articu-

lation by means of slips derived from the deep flexor tendons of the affected fingers. These were sutured together on the extensor aspect of the finger and thus prevented the hyperextension of the metacarpo-phalangeal of the joint.

Fixation of the Sacro-Iliac Joint.

DR. C. W. B. LITTLEJOHN (Melbourne) read a paper in which he discussed the fixation of the sacro-iliac joint. He said that the conditions suitable for operation, provided there were no definite contraindications, were subluxations or displacements which recurred after reduction and belt support for six months, chronically relaxed joints not cured by belt support for six months, ligamentous injuries not cured by manipulation, traumatic arthritis, toxic arthritis, tuberculous arthritis, septic arthritis. Dr. Littlejohn described the earlier types of operation carried out on the joint and those which had been involved later. He referred to the operations of Picque, Wheeler, Albee, Arnold, Gaenslen, Smith-Peterson, Verrall, Tuffier. He said that as far as the choice of operation was concerned, an extraarticular method would be preferred in all tuberculous and septic forms of arthritis, with possibly a secondary attack on the actual focus. In mechanical conditions intraarticular fixation, giving a larger and presumably firmed area of bony ankylosis, was desirable. At the same time good results had been claimed in mechanical conditions by extraarticular fixation. As far as after treatment was concerned, the patient lay on a firm bed with a minimum of movement for four or five weeks and then got up and about. He reported a series of twenty-one operations performed on twenty-one patients out of a series of seventy-six suffering from sacro-iliac conditions of various sorts. Of the seventy-six patients, five had suffered from fractures, fifteen from subluxations of an acute nature, five from recurrent subluxations, forty-seven from chronic strain and traumatic arthritis, two from septic arthritis and two from tuberculous arthritis. Two of the patients with recurrent subluxation had been operated upon by the Smith-Peterson method. Fifteen of those with chronic strain and traumatic arthritis had been operated upon, one by the Picque method, one by the Gaenslen method and thirteen by the Smith-Peterson method. Two patients with septic arthritis had been operated on, one by the Verrall method and one by the Picque method. The two with tuberculous arthritis had been operated on by the Smith-Peterson method. In fourteen instances the results of operation had been good. One patient operated on by the Smith-Peterson method had improved, but the result was not good. Another operated on in the same way had not been traced, but the immediate result had been good. A third operated on in this way had had a moderately good result, but other injuries had retarded recovery. In a fourth fixation was good, but there had been a mistaken diagnosis. One patient operated on by the Picque method had a moderately good result and drove a cab. One patient operated on by the Verrall method had had an immediately good result, but had died under a later operation. In one instance the nature of the result was not stated.

DR. J. V. R. HOETS (Sydney) asked if any patients who had been operated on for fixation of the sacro-iliac joint, had subsequently been through a confinement and if so, what was the effect on the subsequent labour.

DR. L. G. TEECE (Sydney) said that in his experience at the Royal Prince Alfred Hospital severe sacro-iliac conditions were much less frequent than appeared from Dr. Littlejohn's paper. The diagnosis was difficult; the functional conditions had to be separated from the organic. He held that many of the former type occurred in workers after some minor injury. Of the organic conditions it was difficult to decide which required radical treatment. He would hesitate to operate on any patient in whom there was no persistent wasting of the thigh on the affected side. X ray examination was of little value in establishing a diagnosis. He had operated only four times and on each occasion had used the Smith-Peterson technique. As a result of his experience with this operation, he disagreed with the idea that hæmorrhage was slight, particularly in fat people or those with a large *gluteus maximus*.

DR. D. J. GLISSAN (Sydney) regretted that the time available had not been spent on a discussion of the symptoms rather than the operative treatment of sacro-iliac conditions. The question should be discussed with radiographers, as he had seen advanced tuberculosis without any radiographic signs. He had seen many patients with less severe lesions than those described by Dr. Littlejohn. He mentioned that obvious X ray appearances did not necessarily disclose the cause of pain in a sacro-iliac joint. He had done only two operations of this type and preferred Verrall's operation which he considered easy. He preferred adhesive strapping applied from the buttocks to the thorax for extra-articular fixation and had found that relief followed its use at an early stage. He did not agree with Dr. Littlejohn regarding the treatment of strains as he considered that the original injury was not reproduced; the adhesions were broken down.

DR. R. V. GRAHAM (Sydney) drew attention to the fact that Albee very rarely found it necessary to operate on patients referred to him for persistent pain which had been diagnosed as being due to chronic sacro-iliac strain. Albee had described many of these conditions under the title of myofascitis and stated that he depended on elimination of the primary cause by adequate treatment of any possible foci of infection, particularly in the bowel. Since adopting this line of treatment he had very rarely performed an operation for sacro-iliac fixation.

DR. L. O. BETTS (Adelaide) would have preferred a discussion of the diagnosis of low back pain. The Americans regarded inflammatory conditions as the cause of this type of pain rather than trauma and more recently Smith-Peterson had also emphasized this view. He could not see how it was possible to have subluxation except in the female after physiological stretching of the ligaments. He considered that none of the commoner supports could immobilize the sacro-iliac articulation efficiently. During fifteen months in England he had seen only two sacro-iliac conditions diagnosed, one of which had recurred after Verrall's operation. He thought that arthritis of the lumbar articulations without arthritis between the bodies of the vertebræ was frequently confused with these conditions. Hibbs had ceased to immobilize any of his arthrodeses.

DR. A. V. MEEHAN (Brisbane) stated that he had often encountered the condition in private practice. Many patients were treated by physicians for long periods and in many the condition was attributed to uterine conditions. He quoted Bonney and Worrall, who considered that gynæcological conditions did not cause low back pain. He found it difficult to decide when to operate and considered that a long trial should be given to conservative methods before operation. Osgood had also emphasized the frequency with which these conditions were due to bowel infections. Dr. Meehan emphasized the point that the bed should not be allowed to sag during the treatment of these patients and he considered that manipulation should always be tried.

Dr. Littlejohn in reply agreed that the majority of the patients recovered if treated for a sufficiently long period, but he suggested operative fixation first and treatment of focal infections afterwards. The only death in his series could not be attributed to the sacro-iliac fixation. He agreed that the X ray examination was valueless in the diagnosis of acute conditions as there was a range of only three millimetres in the movement of the upper part of the joint. None of the patients included in his series had since become pregnant. He always employed a rubber belt for support in the less serious conditions and had noticed that in the last five years many of his patients had had a long history. He had found that a definite tendency to report earlier was becoming manifest and consequently it was nearly always possible to cure the patients by manipulation and treatment for focal sepsis with the result that he performed the operations less frequently than formerly. Hæmorrhage in the Smith-Peterson operation could be avoided by not going through the *gluteus maximus*, as it was possible to get sufficient exposure with good retraction. Verrall's method was easier if a slot were cut in one ilium rather than an

attempt made to drive the bone peg through a hole in each ilium. In treating bowel infections associated with these conditions he preferred abdominal massage, bran and paraffin by mouth rather than bowel irrigations.

Kümmell's Disease.

DR. GORDON SHAW (Melbourne) and DR. L. O'SULLIVAN (Melbourne) read papers on Kümmell's disease.

A discussion followed.

Compound Fracture of the Lower Limb.

DR. FAY MACLURE (Melbourne) read a paper on compound fracture of the lower limb. He said that he proposed to deal with only three aspects of the subject, the problem of the shocked patient, the question of amputation, the treatment of the fracture itself. Out of the study of fractures at the war three great lessons had emerged: the value of warmth, of nitrous oxide and gas anæsthesia and of early blood transfusion. These lessons were being forgotten or disregarded. Dr. Maclure dealt with each of these in turn and laid stress on the necessity for their provision in all hospitals in which shocked patients were admitted after accident. The problem of amputation in the lower limb was much different from that in the upper. In the former an artificial limb might be much superior to a sadly damaged living leg. There were three stumps in the lower limb which were suitable for the fitting of an artificial limb. These were the stump in the lower third of the femur and above the condyles, the thigh stump; the stump below the knee; the stump resulting from Syme's operation. The stump below the knee should contain at least 7·5 centimetres (three inches) and at most the upper half of the tibia. In discussing secondary amputation, Dr. Maclure referred to the parts played by secondary hæmorrhage and sepsis and said that in the performance of the operation the principle borne in mind should be the minimum of trauma and the maximum of drainage. In a primary amputation the surgeon could frequently select the site and by an aseptic operation obtain a healthy stump below the knee, whereas by bad judgement and weak decision he might when sepsis followed an attempt at conservation, be obliged to perform secondary amputation through the knee or thigh and leave his patient at considerable disadvantage with regard to an artificial limb. The primary object in the treatment of fracture was to prevent the development of sepsis. The ideal method of preventing sepsis in a recent wound was by means of an aseptic operation to excise *en bloc* the whole of the damaged skin, fat and muscle containing foreign matter and *débris*. The second basic principle in the prevention of sepsis was the absence of tension about the wound tissues; drainage was essential. Efficient drainage material was one which had practically no dimensions and thus did not encroach upon the capacity of the fibrous sheath and add to the tension of the contents, which adapted itself to the tortuosities of the track of the main wound and which delayed the sealing of the wound. Vaseline was the ideal drainage material. In regard to treatment by chemicals Dr. Maclure said that it was wiser to concentrate on maintaining the antibactericidal power of the tissues than to rely on antiseptics. In regard to splints, the objective was physiological rest to the injured parts and absence of movement and of irritation was to be maintained until the tissues had recovered their resistance and healing had advanced sufficiently to defeat the invading organism and until the field was once more aseptic. Splints should provide means for traction on the lower and counter traction on the upper fragment, afford control of rotary movement at the site of the fracture, support the limb against the action of gravity, allow easy access to the wound for purposes of dressing without the fragments being disturbed, be comfortable and convenient for nursing needs.

DR. G. A. C. DOUGLAS (Brisbane) read a paper in which he discussed compound fractures of the lower limb. He said that these fractures fell into three classes: those that could be converted into simple fractures by primary closure, those that were already definitely infected and those in which amputation was necessary. Dr. Douglas dealt chiefly with primary closure. There were two essentials: early adequate operation and complete and undisturbed rest afterwards. The matters to be considered in primary operation were exposure of bone, treatment of the bone ends, internal fixation of the fractured bone, treatment when a joint had been opened, use of the tourniquet and dealing with the skin. Dr. Douglas discussed each of these in turn. Any piece of bone that hindered thorough cleansing or that would not easily keep in position, should be removed. In regard to fixation of the fracture, the less foreign material left in the wound the better. In certain cases the bone might be drilled and kangaroo tendon used. He did not approve of the use of wire, plates or bone pegs. The use of a tourniquet was not desirable. Active treatment rested between complete Carrel Dakin treatment and free drainage without continuous bathing of the wound with antiseptics. He left this question for discussion.

DR. ARCHIE ASPINALL (Sydney) said that as a result of twenty years' experience at Sydney Hospital he had come to the conclusion that it was very dangerous to close compound fractures, as gas gangrene had been observed many times every year. He always cleansed the limb personally, putting on a large pair of rubber gloves after being prepared for the operation.

DR. HAMILTON RUSSELL (Melbourne) endorsed Dr. Aspinall's remarks. He was convinced if compound fractures were closed, the tension in the wound after forty-eight to seventy-two hours would cause failure. The original aim of Lister to make compound fractures heal as simple fractures was not met by thus closing the wound. A great deal of unnecessary damage was done in compound fractures by excising the skin and other tissues. He referred to the power of resistance of healthy tissues to bacterial invasion. Dr. Maclure had touched the salient point in treatment. He considered the correct principles in the treatment of compound fracture to be purification of the limb, purification of the wound without chemical antiseptics and the insertion of no sutures whatever. The treatment of civil wounds should not be modelled on the treatment of war wounds.

DR. H. GILBERT (Adelaide) described his technique in the treatment of compound fractures in children. He left the wound open in every instance and used rubber tissue for drainage and eusol or saline solution for dressing the wound. In one instance he had attempted to plate the tibia, but ultimately had to remove the plate. Otherwise he used only catgut for uniting fragments. He had used passive congestion for delayed union with satisfactory results.

DR. C. E. CORLETTE (Sydney) agreed with Dr. Hamilton Russell regarding the treatment of compound fractures. Healthy tissue should not be cut away, the projecting ends only of bone were nipped off and the limb should not be opened up. Infection was not likely where there was only a small wound with blood exuding from it. He considered the Carrel-Dakin treatment, if used immediately, would prevent sepsis and result in primary healing. It was the only type of treatment he employed for compound fractures. Compound fractures should not be plated.

DR. V. M. COPPLESON (Sydney) supported Dr. Douglas's opinion. He stated that in the majority of compound fractures it was possible to close the wound, which was partly the result of indirect injury. He had done this about twenty times. He cleansed the wound, washed it with spirit and removed merely the skin edges. The fracture was reduced and the limb placed in plaster with a window over the wound. Primary healing had occurred in each instance when he had practised primary closure. In one patient the internal saphenous vein had been seen running across one corner of the wound. The vein had been avoided, but the patient had died from thrombosis on the eleventh day.

DR. S. C. FITZPATRICK (Hamilton) had observed gas gangrene even when wounds had been left open and considered the fact of its occurrence as an argument in favour of more complete removal of devitalized tissue rather than in favour of the abandonment of the closure of these wounds. Few recent graduates had seen typical gas gangrene. He suggested drainage of compound fractures for twenty-four hours in order to avoid tension.

DR. JOHN KENNEDY (Melbourne) considered that the ends of the bone should not be cut off in any compound fracture. He drew attention to two patients treated in this way in whom severe osteomyelitis developed.

DR. D. J. GLISSAN (Sydney) considered that it was impossible to lay down any definite rule of treatment for all compound fractures. He agreed with Dr. Maclure that the blood pressure was the best guide in deciding when to operate in shocked patients and suggested that the Carrel-Dakin technique should be carried out in its entirety.

DR. VICTOR HURLEY (Melbourne) drew attention to the two different types of compound fracture in the lower limb, those in which the wound in the skin was due to penetration from within and those in which the wound was due to crushing violence with a gross comminution of fragments and in which it was not possible to suture the periosteum or skin.

DR. H. SKIPTON STACY (Sydney) considered that the apparent differences of opinion expressed by various speakers were due to the fact that they were really talking about different types of compound fracture. He realized that war practice was not as a rule applicable to civil practice. He emphasized the value of dependent drainage in all compound fractures of the lower limb. This was effected by making a stab wound in the posterior aspect of the limb and inserting a drainage tube. The Carrel-Dakin technique should be applied thoroughly when it was used. When there was much displacement he applied skeleton extension by Schmerz hooks to the *calcaneus* with the leg resting on Corlette's modification of the Hodgen splint.

Dr. Fay Maclure in reply thought he had made it clear that there were these two different types of fractures; in the perforated fracture it was just as important to do an open operation as in any other. He disagreed with Dr. Corlette regarding the nipping off of projecting ends of bone, as the operator could not know what organisms had been introduced into the wound. The use of bismuth-iodoform paraffin paste on skin edges and the underlying tissues provided free drainage even though the wound had been sutured.

Dr. Douglas in reply said there was always a considerable amount of bleeding through the edges of the wound even after suture. This allowed sufficient exit for the exudation even when the leg was in plaster and so prevented excessive tension. Patients with compound fractures should be operated on as soon as shock would allow; this treatment frequently resulted in the avoidance of infection. Removal of loose fragments of bone was not very satisfactory, but it was necessary to apply any method in a large series in order to assess the value of the results.

Injuries to the Semilunar Cartilages.

DR. LENNOX TEECE (Sydney) read a paper on injuries to the semilunar cartilages. He based his remarks on twenty-seven patients on whom he had operated. He emphasized the importance of taking a careful history and of not accepting the patient's statement that the joint had become locked. It was often necessary to send the patient away to await a return of the symptoms. As a rule the history was of more assistance in the diagnosis than the physical signs. Save in exceptional circumstances he did not think that operation should follow the first displacement. After three displacements operation might be undertaken. It had to be remembered that repeated displacements over a long period of time were apt to result in the development of a traumatic osteoarthritis and that operation then would yield very disappointing results. In his series 21 had been due to lesions of the internal cartilage, four had been due to lesions of the external cartilage and two had been due to cysts of the external cartilage. In the last two the relationship to trauma was doubtful. Of the 21 internal cartilages details as to the exact nature of the lesion were obtainable in 17. Two had been torn from their anterior attachment, 13 had possessed a longitudinal tear, one had been split transversely and one had been so torn and ragged that it had been impossible to determine its classification. Of the 13 longitudinal tears three had involved the posterior part of the cartilage alone. Of the 27 patients all had returned to their occupations, but four, and these had been but recently operated upon. Labouring men as a rule required from eight to ten weeks before they could resume work.

DR. D. S. WYLIE (Palmerston North) considered the early use of physiotherapy and early instruction in re-education of the *vastus internus* were essential in the after treatment of patients with injuries to the semilunar cartilage to secure an early return to work. Longitudinal tears of the cartilage predominated. The difficulty with the tourniquet could be overcome if it were boiled before the operation and if the surgeon wore two pairs of gloves for its application and discarded the outer pair before commencing the operation.

DR. L. O. BETTS (Adelaide) suggested the use of an Esmarch's bandage instead of a tourniquet, as it could be applied by anybody in the theatre. He had experienced difficulty in making a certain diagnosis after the reaction associated with the injury had subsided. The history was an important feature and he considered that localization of tenderness was useful in differentiating between injuries to the medial and lateral meniscus. These injuries were common in miners who worked in the kneeling position. He considered it essential to remove the whole meniscus when operating upon it.

DR. JOHN KENNEDY (Melbourne) mentioned the difficulty in diagnosing injuries to the semilunar cartilages of the knee, many of which were missed in a mistaken diagnosis of synovitis and many confused with contused subpatellar pads of fat, contused anterior crucial ligaments or synovial fringes. The results of operation were not always good, as there was sometimes definite disability, pain, wasting or recurrent synovitis following operation.

Dr. Teece in reply agreed that delayed convalescence was due to lack of early physiotherapy or to too prolonged rest in bed. He did not agree with Dr. Betts that localized tenderness was invariably a good diagnostic point, as he had met several patients with undoubted cartilage injuries who had located their pain over the medial lateral ligament. Bad results after operation were generally due to the fact that osteoarthritis had supervened on long standing injuries or that repeated effusions resulting in general ligamentous laxity of the joint had occurred or that the operation had been improperly performed.

Talipes Equino-Varus.

DR. W. KENT HUGHES (Melbourne) read a paper on *talipes equino-varus*. A short discussion ensued.

Manipulative Surgery.

DR. E. B. M. VANCE (Sydney) read a paper on manipulative surgery. He said that the cardinal symptoms of adhesions in a joint included the limitation of a particular movement, pain when the adhesion was stretched, muscular weakness and wasting in the surrounding muscles and the presence of one or more definitely tender spots in the joint. It was essential that a radiological examination of the joint should be made in order to prevent manipulation of a bony ankylosis, of a joint in which the articular ends of the bone had been destroyed, of a tuberculous joint, of the joint of a patient with *myositis ossificans* or of a joint blocked by the attached piece of bone, such as a fractured tibial spine in the knee joint. There was a definite series of movements for each joint and these should be fully known before any attempt at manipulation was made. Patients suitable for manipulation were generally those suffering from a lesion which was the result of trauma, but this was not always the case. He divided trauma affecting a limb or the spine into two categories, the one in which violence was applied as a "stub blow," and the one in which it was applied in some form of a wrench. A "stub blow" of a severe nature would produce a crushing of cancellous bone and the inflammatory process of repair gave rise to excessive pain. There were four sites for this type of injury; these were the shoulder, the spine, the subastragaloid joint and the joint of the great toe. Violence applied as a wrench produced four types of injury; it might produce injury to the capsule alone, injury to the capsule and the ligaments, sprains and

partial ruptures of surrounding muscles and dislocations. In these four types of injury a large proportion of those suitable for manipulation was found. Dr. Vance emphasized the necessity for the exclusion of active inflammation before any movement was undertaken in osteoarthritic joints.

Dr. E. J. Glissan (Sydney) thought that few lesions were responsible for so much disappointment as lesions about a joint. The presence of lesions was quite obvious in joints which had been the site of severe injuries, but those following mild injuries were frequently overlooked. Small degrees of limitation of movement were frequently elicited by careful comparison between the functions of corresponding joints. He never hesitated to manipulate a joint which was the seat of osteoarthritis, if there were no obvious signs of an active lesion and he preferred to do this without an anæsthetic.

Dr. W. Kent Hughes (Melbourne) mentioned that Sir Robert Jones considered that one movement only should be made in breaking down adhesions about a joint.

Dr. C. B. Littlejohn (Melbourne) asked what success Dr. Vance had achieved in the manipulation of tennis elbow and were there any special movements to be observed in manipulating the knee joint.

Dr. H. Gilbert (Adelaide) asked how much force was permissible in attempting to manipulate a joint. He spoke of a patient with ankylosis after gonorrhœal arthritis in whom manipulation of the joint had been unsuccessful. A very useful range of movement had been obtained after the patient had accidently fallen on it.

Dr. Vance in reply said that Dr. Kent Hughes could not have appreciated the point of Sir Robert Jones's advice properly to perform one movement only in manipulating a joint. He had intended to convey the impression that there should be one complete movement in each particular direction, but that repeated movements were inadvisable. It was sometimes desirable to avoid complete manipulation at one sitting. The condition of tennis elbow was not always due to adhesions and consequently all cases were not amenable to manipulation. Accurate diagnosis was necessary to decide which joints were suitable; if a source of toxic absorption was present, this also required attention. He would perform manipulations for sacro-iliac sprain after fixation, but would not do so for traumatic arthritis in the same joint. After reading Sir Henry Barker's autobiography he had been unable to elicit his principles of treatment, but thought that his success with knee joints was confined to those conditions in which adhesions were present. He considered that Dr. Gilbert's case was an exceptional one.

Arthrodesis of the Knee.

Dr. A. V. Meehan (Brisbane) read a paper on arthrodesis of the knee joint. He said that this operation should be undertaken when the joint after anterior poliomyelitis was so unstable that the patient had to hold the leg near the knee and use it as a crutch in walking, and also when the patient was constantly stumbling as a result of paralysis of the *quadriceps extensor femoris* and the hamstrings were not strong enough to warrant the transference of their insertions into the patella. The operation was also suitable for the unstable knee joint due to rupture of ligaments when persistent abnormal mobility and pain on walking were present, in certain cases of persistently painful osteoarthritis when any obvious cause of internal derangement had been removed, in tuberculosis of the joint when the condition had not advanced sufficiently far to warrant free excision. The anatomical conformation of the joint tended to defeat the object of the operation and the presence of synovial fluid appeared to have an inhibitory effect on callus formation. The most satisfactory type of operation in his hands had been one in which a large graft 7·5 centimetres (three inches) long was removed from the upper part of the crest of the tibia and inserted into the lower end of the femur and the upper end of the tibia. Dr. Meehan gave details of this operation.

Dr. L. G. Teece (Sydney) asked for an explanation of the exact manner in which the graft was inserted into the tibia. This operation as mentioned by the President was generally an operation of choice, but he was becoming more convinced that it was an operation of necessity in tuberculosis of the knee in adults, as conservative treatment was unsatisfactory. The indications for arthrodesis of the knee in poliomyelitis were very limited because of other contraindications in addition to the flail knee.

Dr. D. J. Glissan (Sydney) asked if the operation was performed for tuberculosis of the knee in children. He had been gratified at the amount of function resulting after arthrodesis and had decided in future to use a curved incision down the inner side of the knee and to remove the tibial spine in order to get better exposure. He thought that the knee should be fixed in slight flexion. He was indebted to Dr. Meehan for the suggestion of using the patella in the form of bone chips as it was difficult to fit a bone graft when it was desired to get flexion.

Dr. Fay Maclure (Melbourne) mentioned that Dr. Hamilton Russell invariably performed arthrodesis for tuberculosis of the knee in young adults and the late George Adlington Syme had advocated amputation of the knee for tuberculosis in patients over fifty years of age. Dr. Maclure considered that the sooner the knee was ankylosed, the sooner would the patient be able to return to his work. He asked what difference existed between arthrodesis and excision. When pain persisted after a long time and there was radiographic evidence of tuberculosis, he considered that arthrodesis was definitely indicated, but he had always excised the joint and had not performed the more conservative arthrodesis. The U-shaped flap across the patellar ligaments had given sufficient exposure, as he opened the knee early in the operation and then turned down and preserved all the soft structures covering the upper edge of the bone. These were subsequently used for a buttress to the excised joint. He was interested to hear of the use of the patella, as it was his custom to leave it *in situ* after removal of the articular cartilage. He agreed that slight flexion was the correct position and reported a case of a patient with two stiff knees, one in the extended position and one in 15° of flexion. The former was painful and inconvenient, but the latter was perfectly free of pain and enabled the patient to sit in public conveyances with greater comfort.

Dr. Gordon Shaw (Melbourne) agreed that function after arthrodesis was extraordinarily good; he had observed that firm union was definitely delayed for a considerable time. In men following a labouring occupation it was the treatment of choice, but he raised the question whether the caliper in women would not give more comfort. He was strongly of the opinion that arthrodesis should be done for tuberculosis in adults, but deprecated its use in children as time was not such an important factor and the risk of general tuberculosis ensuing was worth considering. It was essential in dealing with tuberculosis to remove the tissue, hence excision was indicated rather than arthrodesis. The broad surfaces of bone eliminated the necessity of bone grafting. He also thought that arthrodesis was undesirable in pyogenic conditions. Amputation should be considered in tuberculosis, although he did not frequently practise it. In adddition to slight flexion of the excised knee, he thought slight shortening was also desirable, but he still used a nail, driven up from below through the head of the tibia for fixation and had known no ill results from the use of this method.

Dr. Meehan in reply stated that there was sometimes a real difficulty in inserting the tibial graft. If possible he hammered the graft through the dead centre of the epiphysis of the femur, preparing the bed with a sharp gouge. It was sometimes necessary to split the bed in the tibia for its reception. He had so far observed no subsequent deformity following the use of the graft through the epiphysis. He agreed it could be employed with advantage to some patients with poliomyelitis. He had had no difficulty in getting good exposure or in clearing the tissues through the usual horse-shoe incision and his early remarks showed that he favoured fixation in a position of slight flexion. He was very interested in the views of the Melbourne surgeons who saw more tuberculosis

than he was able to see in Brisbane. He had performed arthrodesis in an adult over fifty, but so far the bone was fusing slowly. He considered that the production of definite lesions in the guinea-pig after the injection of fluid or tissue abstracted from the joint was a necessary criterion for the diagnosis of tuberculosis. In the Hibbs clinic it was the routine to perform biopsy, but he considered that they should complete the operation of arthrodesis, if necessary before starting an autopsy as there was a definite tendency for the tuberculous tissues to extrude itself from the incision after biopsy. He considered that arthrodesis was preferable to excision, as very accurate apposition of the bony surfaces was secured. He advised arthrodesis in adults only for tuberculosis. He mentioned that Hibbs performed this operation on children at any age. He did not consider that it was necessary to remove all tissue in tuberculosis of bone, as the bony union resulting from arthrodesis was frequently perfectly sound without this measure.

Perthes's Disease.

DR. R. V. GRAHAM (Sydney) read a paper on Perthes's disease in which he outlined some experimental investigations on goats. He had divided the right *ligamentum teres* in fourteen goats and had allowed the left ligament to remain intact as a control. The animals had subsequently been killed and the heads of the femurs carefully examined. He had found that the changes varied with the age of the animal at the time of operation. They also varied with the time between operation and removal of the head of the bone for observation. The changes had been almost negligible in goats over the age of six months. In younger goats necrosis and absorption of bony trabeculæ had ensued in an area underlying the attachment of the *ligamentum teres* to the head of the femur; the area had had the shape of an inverted cone. In most instances there had been a definite flattening of the epiphyseal plate in sagittal section. This had apparently resulted from a disproportion between the rate of growth of the cartilage cells on the two sides of the epiphyseal plate; the suggestion was that this led to deformation of the cap similar to that in the early stages of Perthes's disease. The changes had tended to undergo repair when the goat was allowed unrestricted liberty on an ordinary diet. It was obvious that the *ligamentum teres* diminished in importance as a source of blood supply to the femur as the age of the animal increased. Dr. Graham intimated that he was engaged in experiments of a similar nature in which artificial infections with attenuated cultures of microorganisms were used and in which there was some interference with the calcium metabolism.

Bone Tuberculosis.

DR. C. W. B. LITTLEJOHN (Melbourne) discussed the operative treatment of bone tuberculosis. He said that in weight-bearing joints the only satisfactory cure was that obtained by ankylosis. The only satisfactory form of ankylosis was bony ankylosis and in joint tuberculosis uncomplicated by secondary tuberculosis, bony ankylosis was practically unknown. If this opinion were held, the essential factors were an accurate diagnosis and an operation to produce bony ankylosis. The objections were the operative risk, the risk of dissemination, the risk of interference with growth. The immediate operative risk was about 1%. Early dissemination did not occur when the diseased area could be fairly well eradicated. When later dissemination occurred, it was not due to operation, but to the general infection. The risk of interference with growth could be avoided by careful technique. Dr. Littlejohn laid stress on the importance of an accurate diagnosis. The patient should be put to bed without splints or plaster. If after two or three weeks the diagnosis was still obscure, if the joint were accessible, a piece of synovial membrane should be excised for examination. If a decision could not then be arrived at, conservative measures should be adopted. Dr. Littlejohn described the operations suitable for the knee joint, the hip joint, the sacro-iliac joint and the shoulder joint. He had operated on eighteen patients with tuberculosis of the knee joint by the method of Hibbs. In seventeen the final result had been good with firm bony ankylosis and in one amputation had had to be performed. Two patients with tuberculosis of the hip joint had been operated on by the Hibbs method, one by the Albee method and one by the Farrell method. In two the final result had been good, one patient still had discharging sinuses and one could not be traced. Two patients with tuberculosis of the sacro-iliac joint had been operated on by the Smith-Petersen method and one by the Arnold method. In two the final result had been good and one had died of pulmonary tuberculosis three years later. Two patients with tuberculosis of the ankle joint had been submitted to operation; the result in one had been good and in the other bad. Four patients with tuberculosis of the tarsus had been operated on; in three the final result had been good and in one it had been bad.

DR. C. J. O. BROWN (Melbourne) read a paper on the conservative treatment of bone and joint tuberculosis. He confined his remarks to the principles of treatment of tuberculosis, affecting the spine, the hip and the knee. Dr. Brown described the deformity associated with tuberculosis of the spine and discussed the treatment by recumbency and the subsequent use of a steel and leather support. He said that superficial abscesses that were not progressing, should be left alone. If they tended to increase in size or if there was any likelihood of perforation, they should be aspirated. If the condition did not respond to recumbency, operative interference might be indicated. A retro-mediastinal abscess should be emptied or drained. A laminectomy was indicated when there was definite evidence of bony pressure on the cord. Opinions differed as to the value of fusion operations for paraplegia. In regard to tuberculosis Dr. Brown gave the results of treatment in seventy-one patients in whom treatment had been carried out at the Children's Hospital and the Austin Hospital, Melbourne. In twelve instances the diagnosis had been uncertain and a normal joint had resulted from treatment. The result in sixteen instances had been good from a functional point of view. Two patients had had a certain amount of movement; in five fibrous ankylosis and in nine bony ankylosis had been present; in nine instances the result had been unsatisfactory, thirteen patients had died and twenty-one were still under treatment. The patients with a good functional result had been under treatment for periods varying from fifteen months to nine years. All except four had suffered from abscesses at some stage and in eight sinuses had developed. The most satisfactory treatment of tuberculosis of the knee joint was an operation of arthrodesis combined with heliotherapy. Non-operative treatment led to quiescence and a partially immobile joint in most instances, but the liability to relapse was always present. Dr. Brown had never seen firm ankylosis result from non-operative treatment.

DR. R. M. DOWNES (Melbourne) read a paper on the criterion of cure in tuberculosis of bone. After four years' observation of patients with tuberculosis treated by heliotherapy, he had come to the conclusion that this period was too short to allow any information of value to be obtained. He discussed the subject from the point of view of tuberculosis of the spine and that of the hip. In arriving at a decision in regard to cure of the former condition four factors had to be considered: the results of physical examination, particularly in regard to muscular rigidity, the general health and spirits of the child, the interpretation of skiagrams and the standard of time. Absence of muscular rigidity on movement was essential before the patient was ready to advance to the ambulatory stage. The fact that the patient was in the highest spirits and showed every evidence of being in good health was *per se* no indication of healing, for this commonly occurred before healing had taken place. In regard to the interpretation of skiagrams it was necessary that X ray examination should be made at frequent intervals and the orthopædic surgeon should be able to form his own opinion without necessarily accepting the report of the radiologist as if it were an answer to a mathematical problem. Dr. Downes confessed to a considerable degree of scepticism as to the correctness of the opinions put forward by radiologists in regard to the activity or quiescence of

spinal tuberculosis. In regard to the time factor it was very difficult to come to a satisfactory general conclusion. The experience of Dr. Downes was that a year and a half for the Victorian climate was the minimum in the most favourable conditions only and that more generally the period would be two years. In regard to tuberculosis of the hip a decision should be made by the combined consideration of the general condition, the local signs and X ray appearances.

DR. R. B. WADE (Sydney) considered that the operative treatment of tuberculosis was entirely new and it would take many years of work before they could arrive at a true opinion of its value. He did not question that a higher altitude was better in heliotherapy, but at an inland convalescent home previously attached to the Royal Alexander Hospital for Children the children had progressed so badly that it had been abandoned. The monotony of hospital life apparently was responsible for the failure. He found that children frequently improved after removal from the hospital to their home, even though the conditions in the home were not good. The great variety of interest provided at the seaside was a great advantage. As his experience increased he found a greater tendency to prolong the period of recumbency, particularly in the seaside home, up to two or three years. As a criterion of improvement he placed great value on X ray examination, together with the clinical quiescent condition for at least three months. He considered it was necessary to adopt uniform X ray technique before a correct opinion could be formed regarding the contrast between the skiagrams taken consecutively of any one patient. The abduction and extension maintained in the treatment of tuberculous conditions of hip were soon overcome by muscular pull on the soft tissues when the patient started to walk.

DR. H. GILBERT (Adelaide) considered that secondary infections in abscesses were probably blood borne and not communicated from extraneous sources.

DR. L. O. BETTS (Adelaide) thought there was a great danger of overdoing heliotherapy in Australia. He considered the chief benefit from heliotherapy was due to the stimulating effect of cold air on the skin rather than to the effect of ultra-violet light rays. He found it difficult to prevent patients from being overheated when undergoing heliotherapy, unless they were protected in the shade. No doubt a good result was obtained in children by long conservative treatment and he did not consider surgical treatment was necessary, but even the conservative Liverpool school agreed that surgery was indicated in adults and that it was frequently necessary to amputate the leg for tuberculosis of the ankle in adults.

DR. E. B. M. VANCE (Sydney) considered that the ideal result was represented by true bony ankylosis of the trabeculæ rather than through the diseased portion. They had to approach the question of activity tentatively and not to give any definite prognosis until after the patient had been allowed to be active and the behaviour of the joint had been observed. He considered it better to avoid extension of the hip and sometimes to get a definite kyphosis in the effort to favour bony ankylosis. Operative treatment of tuberculous joints was not new as it had been tried before in France. They would pass through the experience and return to conservative measures.

DR. G. SHAW (Melbourne) maintained that the operation was only an incident in the treatment of tuberculosis of bones and joints. It was important to remember that the tuberculous lesion was not cured by operation, especially in young children. He considered it advisable to excise as much as possible of the tuberculous focus as convalescence and healing were more rapidly obtained. There was a grave risk of failure if an attempt were made to place a bone graft into a tuberculous bone. He had seen patients with undoubted tuberculosis of joints recover with an astonishing degree of movement and considered that firm bony ankylosis was not obtained in a joint with a pure tuberculous infection. The ankylosis was always due to a superadded infection. He had also seen patients with joints which appeared to be ankylosed on clinical and radiographical examination, but which possessed a surprising degree of movement under anæsthesia. Regarding criterion of cure, he preferred to use the term arrest rather than cure, as it was difficult to be sure that these lesions were ever cured. Sir Robert Jones had obtained good results in the treatment of tuberculous joints in the slums of Liverpool by the simple expedient of putting the patients out in the fresh air.

DR. D. S. WYLIE (Palmerston North) was interested to find that the same difficulties regarding the interpretation of skiagrams existed in Australia as he had observed in New Zealand and emphasized the fact that the absence of visible evidence should not be regarded as conclusive proof that there was no disease.

DR. D. J. GLISSAN (Sydney) also considered that no radiologist could state whether a joint was ankylosed or not. The man who operated after careful selection, would get the best results, as conservative measures also had their place in the treatment of this disease. He did not accept Rollier's teachings and findings unreservedly. He considered that a slight degree of abduction was advantageous in tuberculosis of the hip as it allowed the limb to swing clear of the ground.

DR. A. V. MEEHAN (Brisbane) said that Hibbs aimed at slight abduction and 30° of flexion and the functional results were so good that it was difficult to distinguish an ankylosed hip in this position. He emphasized the need for routine investigation of patients with tuberculosis of a joint, as renal infection was frequently found in such patients if it were looked for.

Dr. Littlejohn in reply stated that some years previously he had collected as many records as possible in the Melbourne Hospital of tuberculous disease of bones and joints in adults. In 55% there was evidence of a multiplicity of lesions. It was impossible to distinguish which were due to bovine and which to human bacilli. He always interpreted the term heliotherapy to mean exposure to light and fresh air rather than to actinic rays of the sun.

Dr. Brown agreed with Dr. Gordon Shaw's contention that tuberculosis was a general disease rather than a local disease when it attacked a bone. When apparent cure resulted with some mobility in a hip joint, he thought that it was really an arthroplasty which had been well done by Nature and in his opinion articular cartilage could not survive when the skiagram yielded evidence of erosion. He did not agree with Dr. Glissan's contention that abduction was advantageous since, if one hip were abducted to 15°, the other must be adducted to a similar extent for the patient to walk. In reply to Dr. Downes he said that it was impossible to get abduction if the patient were treated in an ambulatory splint. Extension was not essential, but the dangers of extension were not apparent if it were combined with abduction, as it was impossible to drag the bone surfaces apart in this position. A combination of the two positions enabled the surgeon to prevent pressure on the acetabular shelf and to avoid subluxation. In reply to Dr. Gilbert he expressed the opinion that the cavity was the important feature in such infections rather than the infection. A sinus leading into such a cavity would allow organisms to flourish in it. He admitted that blood infection sometimes occurred.

Dr. Downes in reply said he considered that a sinus did not always indicate activity of the disease. He appreciated the support accorded to his views regarding the relative value of skiagrams and recalled Dr. Hamilton Russell having said many years previously that if clinical findings disagreed with radiographic findings, he preferred to disregard the latter. He suggested a lateral stereoscopic view as being more likely to give information in disease of the spine than any other. He did not consider that ankylosis was the ideal result, since mobility was attained in a certain number of patients. He agreed with the attitude that it was dangerous to overdo heliotherapy, but did not agree that every tuberculous infection was generalized. Dr. Webster at the Children's Hospital had not found any bovine bacilli in bone or joint lesions. The only instances in which bovine bacilli had been isolated, were in disease of the tonsils and the cervical glands.

Wrist Drop.

DR. LOCKYER POTTER (Melbourne) discussed the functional results of tendon transplantation for wrist drop. He said that the value of the operation appeared to depend largely on the extent to which the transplanted muscles might act as extensors in physiological harmony with the remaining flexors. The possibility of the muscles so acting would be gauged by the ability to acquire in a normal limb the power to contract one flexor of the wrist while relaxing the other. It had been found that well directed mental effort had not been instrumental in obtaining this result. It had to be recognized that the functional result would be determined almost entirely by the operating surgeon, that the help received from after-treatment would be very little and that the movement obtained in the hand would be to a preponderating degree due to the action of the normally placed flexor muscles contracting against an almost passive transplant. Dr. Potter went on to show that the functional result depended on the tension of the transplant. He described in detail the movements which were possible when the transplant was under such slight tension that, when at rest with all the muscles relaxed, the hand occupied a position corresponding to 30° of flexion at the wrist joint. In that position gentle flexion of the fingers elicited no responsive contraction of the transplant, probably because its optimum length was exceeded and slight movements such as picking up pins could be carried out. He showed that when the tension of the transplant was increased, the range of movement diminished.

DR. L. O. BETTS (Adelaide) asked if Dr. Potter had used the *flexor carpi ulnaris* as well as the *flexor carpi radialis* for these transplantations.

DR. D. S. WYLIE (Palmerston North) stated that in his experience the most satisfactory results in tendon transplantation were due to subsequent restoration of function in nerves which had been previously injured. He considered that better functional results would be obtained by transplanting both flexors.

DR. C. B. LITTLEJOHN (Melbourne) found it better to fix the wrist joint by an extra-articular bone graft and then to transplant the tendons of the *flexor carpi ulnaris* and *flexor carpi radialis* into the extensors of the fingers.

DR. A. V. MEEHAN (Brisbane) stated that he had transplanted the *pronator radii teres* into the wrist extensors, the *flexor carpi radialis* into extensors of the thumb and the *flexor carpi ulnaris* into the common extensors of the fingers and had been surprised at the good results so obtained. He fixed the hand midway between the horizontal position and complete dorsiflexion. Very little reeducation was required. He thought that the poor results emphasized by Dr. Lockyer Potter in the technique which had been followed, were caused by the use of a tendon which was inadequate for the work it was asked to perform.

Dr. Lockyer Potter in reply said that he had seen both flexors of the carpus transplanted only twice. The one operation had been complicated by a nerve suture and the result could not be attributed to the tendon transplantation alone. In some patients the nerve had recovered its function after suture and subsequently the hand had been handicapped by the transplant and it had to be undone. The bone graft at the wrist suggested by Dr. Littlejohn overcame one portion of the problem of extension of the wrist and allowed the transplant to operate at better mechanical advantage than the common extensor tendons; without such a procedure extension of the wrist required a powerful muscle. He suggested the use of photographs to check the results.

SECTION OF OPHTHALMOLOGY.

President's Address.

DR. D. D. PATON (Perth) in his address as President of the Section of Ophthalmology, traced briefly the early history of ophthalmology up to the middle of the nineteenth century. This period, he said, saw the opening of another epoch in which the systematized knowledge of refraction was to become a matter of daily application and the word asthenopia was to pass into common use. He described the introduction of the use of the ophthalmoscope and the advent of a period of precision and measurement with the ophthalmometer, scotometer, tonometer and perimeter. After referring to the advance made in ophthalmic surgery, he said that the elucidation of biochemical processes appeared to be the probable starting-off point for the next advance. Biochemical processes must underlie certain pathological processes and without an understanding of them preventive work in, for example, certain forms of glaucoma and cataract, would be as impossible as antibacterial therapy in the prebacteriological days. Dr. Paton referred at length to the question of myopia and concluded that its explanation awaited the biochemist. There was a necessity for closer cooperation with medicine and this applied more particularly to neurology. Parson's analysis of vision into protopathic and epicritic elements provided a working hypothesis to the understanding of a difficult subject. It enabled them to differentiate rods and cones by their preganglionic connexions instead of morphologically and to follow one class of impulses from the phylogenetically older ventral portions of the lateral geniculate body towards the superior colliculus and the other through the dorsal portion of the same body towards the visual cortex. The one disturbance of a primitive reflex in which the phylogenetic inquiry might assist clinical observation in suggesting direction to pathological investigation, was the reflex iridoplegia described by Argyll Robertson. Dr. Paton made reference in this connexion to the work of Ingvar and to the views of Kinnear Wilson. By research into postural reflexes Sherrington, Magnus and de Kleijn had advanced the knowledge of function. They had demonstrated the independence in the lower animals of the visual aids in maintaining equilibrium and also the growing importance of the visual cortex in the optical righting reflexes of the higher animals. These researches had also brought into prominence the importance of the proprioceptive impulses from the muscles of the head and neck which played a large part in the complex mechanism of spatial perception, and this became a synthesis of visual, labyrinthine and proprioceptive stimuli or more shortly of visual-postural receptions with recombinations of protopathic factors. Dr. Paton went on to discuss the production of binocular vision in an infant and that of strabismus. He dwelt particularly on the inhibition of muscles after the withdrawal of the stimulus and referred to Pavlov's work in this connexion. Investigations of reflexes increased the number of reflex actions at the expense of those that had formerly been considered voluntary, and through them attacked the problems of psychology. It was certain that syntheses of stimuli and their reflexes were built upon preceding syntheses in ever-increasing complexity until the greatest synthesis of all, a concept, was reached. A presiding centre for associations did not apparently exist and the seat of the concept was undetermined, but they were one step nearer its revelation. It thus appeared that existing problems might be viewed from different points of view and much remained to be done. In conclusion Dr. Paton referred to the question of opticians. He saw no harm in pedlars hawking spectacles. The man who did the harm was not the pedlar, but the man who aped a knowledge that he did not possess and who had a smooth, persuasive tongue. Let opticians agree to eliminate misleading advertisements and eschew the methods of the patent medicine vendors. Occulists could assist in maintaining the standard of the opticians on whose good work they depended.

Slit Lamp Microscopy of the Anterior Chamber.

DR. GRANVILLE WADDY (Sydney) read a paper on slit lamp microscopy of the anterior chamber. He confined his remarks to discussing the tissues which could be seen, whether they were in the normal or pathological state. He said that the literature on the slit lamp was largely confined to the findings disclosed; the time would surely come, however, when interpretations and meaning of the details would be available. Dr. Waddy took each of the structures of the eye in turn and described the normal appearances and those associated with many pathological states. He placed particular emphasis on the value of

the lamp in early sympathetic ophthalmia. He quoted Duke Elder's statement that if the slit lamp had no other value than that of providing the opportunity of recognizing sympathetic ophthalmia at an early date, it would justify its cost and the labour expended in mastering its technique.

Dr. E. A. Brearley (Sydney) said that he would like to support Dr. Waddy's remarks in regard to the slit lamp. It gave a superior examination to anything that was yet in vogue. It had enabled them to see certain things for the first time in the history of ophthalmic work, particularly bodies in the anterior chambers and it was extremely interesting to see their behaviour. It was his experience that the circulation of the anterior chamber varied with the progress of the condition. As it progressed, it became undoubtedly slower and as it improved, the circulation increased. He called to mind a case of *siderosis bulbi* in which the anterior chamber was full of opaque particles. He had performed paracentesis. Improvement had been striking. Increased movement of the currents had been observed. The slit lamp was invaluable for cataract examinations before operation, especially when examining for *keratitis punctata*. He had examined first with the *loupe* and then with the slit lamp and its value had been proved in one patient suffering from cataract in whom operation had been unsuccessful. An operation had been performed on the other eye for cataract. No trace of punctate keratitis could be detected with the *loupe*, but was easily discernible by using the slit lamp. He agreed with Dr. Waddy that it should be a part of an ophthalmologist's equipment.

Dr. E. Temple Smith (Sydney) said that he had regarded the clinical value of the slit lamp as limited and had not used it, but after having heard Dr. Waddy's remarks, he realized that in certain circumstances it might be invaluable. Its use in research was undoubted.

Dr. Guy A. Pockley (Sydney) said that he had used the slit lamp and wished to substantiate what Dr. Waddy had said. With all new instruments it required time before their advantages were appreciated. It was difficult to interpret correctly all that they saw. He thought that when the slit lamp had been in use for some years, its clinical importance would be recognized. He held that its use was attended by two disadvantages. Since it disclosed a wealth of detail there was a danger that it might be used to the exclusion of other reliable methods. If this were done, they might overlook many important facts. There was a danger to the patient from the bright light causing irritation, despite the fact that only a small beam was reflected into the eye. He regarded the hand slit lamps as valuable instruments. In clinical work they could see almost everything that was necessary for the institution of treatment with the smaller instrument.

Dr. H. V. Coverdale (Auckland) agreed with Dr. Pockley that important points were liable to be overlooked. In one instance *keratitis punctata* was overlooked through attention being misdirected by the condition of the lens and no *loupe* being used. He advised the use of the *loupe* first and the slit lamp afterwards. He had had a patient, forty-five years of age, with a perforating injury. Fourteen days later the other eye became painful and the tension raised. No punctate keratitis had been seen with the *loupe* and he had thought the pain was due to glaucoma. He had used a slit lamp, but at that time had been inexperienced in its use and mistook the haziness of cells on the endothelium for haze of the cornea due to glaucoma. Had he been familiar with the slit lamp appearances, such a mistake could not have occurred. He advised the examination of the capsule after cataract extraction to see if there was any imminence of prolapse of vitreous. In some instances the occurrence of glaucoma after needling could be avoided by such an examination.

Dr. D. D. Paton (Perth) thought that many conditions diagnosed as acute glaucoma in elderly people were in reality iridocyclitis. The slit lamp would assist greatly in the differentiation. This would also explain why iridectomy failed at times. The slit lamp and microscope might also be of value in differentiating and classifying different types of herpes of the cornea by observation of the appearance of the nerve fibres. Dr. Coverdale had spoken of glaucoma following needling; in many eyes after needling, very large herniæ of the vitreous body were seen without any glaucoma. Dr. Pockley had mentioned the hand slit lamp. Dr. Paton said that he thought that the slit lamp was an improvement in that people use a microscope. Good results were obtained with a corneal microscope when a hand lamp was used and he advised this if a slit lamp could not be obtained on account of the expense.

Dr. Waddy said he was in a way disappointed that all the criticism was favourable to the slit lamp. He was an enthusiast about the lamp. He agreed that its use was restricted to special conditions, but in these it was invaluable. With regard to the President's remarks concerning the use of the microscope with the hand lamp, he would advise the complete instrument despite the increased expense.

Ocular Signs in Neurological Diagnosis.

Dr. A. W. Campbell (Sydney) restricted his remarks to papillœdema and certain disturbances of oculo-gyric crises. Papillœdema was not necessarily present in cerebral tumour, nor was there a constant relation between the size of a tumour and the degree of papillœdema. Dr. Campbell described Stopford's conception of papillœdema. He had put forward the suggestion that the veins of Galen suffered pressure as they curved under the *splenium* of the *corpus callosum*. It could be shown that a tumour of the mid-brain exerted pressure on the veins of Galen. The veins drained the chorioid plexus; obstruction to the veins increased capillary pressure within the plexus. This would lead to an increased pouring out of cerebro-spinal fluid and increased cerebral pressure. He referred briefly to papillœdema caused by lead. He asked the ophthalmologists whether they had any knowledge of an apparatus devised by Baillart, known as the ophthalmo-dynamometer. He pointed out the great advantage that would be derived from any mechanism whereby they would be able to measure intracranial tension without exposing the patient to risk.

Of the disorders of the ocular movement he selected oculo-gyric crises for special discussion. Lesions of the sixth nucleus on one side might paralyse the external rectus and the internal rectus on the opposite side, producing lateral conjugate deviation. Oculo-gyric crises occurred in *encephalitis lethargica* and especially in Parkinsonians. There appeared to be a psychic element in this complex. Typically the crises occurred suddenly and were usually precipitated by an emotional stimulus. They consisted in an uncontrollable turning and fixation of the eyes, usually in an upward direction. At times the fixation was forwards, rarely sidewards or downwards. Upward movement was esentially a human movement and was almost reflex. In conclusion Dr. Campbell spoke of the advantage derived by the neurologist from the assistance given by the ophthalmologist in the diagnosis of lesions such as thrombosis of the calcarine artery. Measurement of the field of vision distinguished between such conditions and hysteria and malingering.

Dr. H. F. Maudsley (Melbourne) read a paper in which he discussed the importance of ocular signs in neurological diagnosis from the clinical aspect. He illustrated his remarks with typical case histories. He refererd to the number of patients in whom varying degrees of pallor of the optic disc were found and in whom there were no abnormal neurological or serological findings. In young adults disseminated sclerosis was the commonest condition in which slight temporal pallor of the discs was the earliest sign. This condition never progressed to a complete atrophy. He also said that when the ocular signs were doubtful, the complete examination of the visual fields might reveal a central scotoma. The earliest sign of disseminated sclerosis might be transient ocular palsies. The Argyll-Robertson pupil was not indicative of neuro-syphilis. He had recently found it in a patient suffering from the after effects of head injury in whom there were no findings indicative of syphilis.

Dr. E. A. Brearley (Sydney) discussed the value of ocular signs in neurological diagnosis. He said that it was becoming increasingly evident that with recent developments in the investigation and explanation of nervous disorders ocular signs were becoming less valuable to the neurologist. The day was long past when a

particular ocular sign indicated a particular neurological diagnosis or when any individual nervous disease had some special ocular sign for its own property. Apart from the increasing number of nervous diseases to which any one ocular sign might be common, the oculist in the course of a number of years found such signs occurring as isolated phenomena without any obvious or ascertainable explanations or relations. In the course of his remarks Dr. Brearley discussed such subjects as nystagmus, lenticular changes in *myotonia atrophica*, pupillary changes and the sympathetic nervous system, the Argyll-Robertson pupil and signs associated with lesions of the visual tract.

Dr. J. Lockhart Gibson (Brisbane) said that in connexion with papillœdema due to acute ocular plumbism they knew much more than they did at first and looked upon it as an acute condition. Acute symptoms were generally thought to be due to gastritis, but really were caused by the ingestion of large quantities of lead in a few weeks, when the patient was affected by headaches, vomiting, papillœdema and paralysis of one or both external recti. He mentioned the history of a patient who had had definite papillœdema thirteen years previously and had given this history. He saw him again in August, 1929, when there was no sign of the previous condition in the fundus; he had perfect health and vision. He thought that the signs were due to irritation of the chorioid plexus from the ingestion of lead. Woollard had found that massive doses of dyes were necessary before dye could be recovered in the cerebral circulation. That was why papillœdema occurred in acute plumbism. Dr. Gibson also stated that he thought that when a large tumour did not cause papillœdema, it was because there was no irritation of the chorioid plexus from broken down brain tissue (hormones) or no blocking of the ventricles.

Dr. H. F. Shorney (Adelaide) said that in regard to slight degrees of optic atrophy, he could assure Dr. Maudsley that the ophthalmologist was as much puzzled as the neurologist. When he eliminated syphilis, he kept disseminated sclerosis at the back of his mind. He spoke of one patient who had had no signs but slight optic atrophy; later he manifested the signs of typical disseminated sclerosis. This was the explanation of some of the cases of slight optic atrophy for which no cause could be found.

Dr. John Bostock (Brisbane) said that he found the discussion of interest and that Dr. Gibson's remarks about lead poisoning were particularly interesting; he would like to pay a tribute to Dr. Gibson's work on that subject. He had not found eye changes in patients with chronic lead poisoning; he had seen whole families who had it in their systems. The disease could be quite easily stopped by not using lead paint. Dr. Campbell had referred to the oculo-gyric crises and left it open whether the causation was psychological or anatomical. Dr. Bostock had been consulted by a girl whose eyes had become fixed when looking at the screen in a picture show. The condition was undoubtedly hysterical. He suggested that in the Parkinsonian syndrome oculo-gyric crises were the psychological reaction to a crippling defect.

Dr. H. M. De Burgh (Sydney) said that he did not think that the ophthalmologists gave the neurologists very much assistance. Papillœdema had many causal factors, for example retro-bulbar neuritis. He suggested that examination of the visual fields formed a better guide to neurological diagnosis than examination of the discs.

Dr. Colin C. Ross (Sydney) asked the advice of the members in connexion with a patient with monocular nystagmus. On covering one eye the vision had become less than $^6/_{60}$ and a rotatory nystagmus had been noted, while when both eyes had been uncovered, vision had equalled $^6/_6$ and the eyes had remained quite stationary. Measurement of the refraction had revealed that the patient had 2·5 diopters of hypermetropia; he had been given glasses. Dr. Ross said he had been very surprised when he saw the patient later when he had been in better health, to find on examination that the nystagmus had become very much less and not so wide.

Dr. D. D. Paton (Perth) said that he thought there was no form of headache which could not come from errors of refraction. Headaches occurred in elderly people with high blood pressure. He did not think that anyone was entitled to say that the headaches were caused by the raised blood pressure. Many of the patients lost their headaches after correction of the error of refraction.

He thought it was very difficult to determine on clinical grounds whether pallor of the discs was progressive or not. In regard to patients who came under the care of the neurologists on account of concussion, it was very important to see if there was any muscular imbalance of the eyes. He had found from experience that it was a very good indication of the progress of these conditions. He referred to a patient concerning whom there had been considerable discussion as to whether the lesion was functional or organic. On taking a chart of the fields a scotoma had been found which undoubtedly pointed to an organic lesion. He had also seen a patient with homonomous scotoma due to antrum trouble; the scotoma had cleared up following treatment of the antrum which was definitely infected.

Dr. H. Coverdale (Auckland) said he would like to suggest that some of the cases of partial optic atrophy were due to vascular rather than to neurological causes. He had had opportunity in England of watching a dozen or more patients with obstruction of the retinal artery and had been impressed with the difficulty of diagnosis when a patient had come for an opinion some weeks or months after one branch of the artery had been obstructed. There would be no ophthalmoscopic evidence of past obstruction beyond some slight narrowing of the artery and some pallor of the disc. Obstruction of one main branch of the central artery might easily pass unnoticed by the patient.

Dr. Campbell said that he had nothing to remark except that he had found the discussion very interesting and instructive. In regard to oculo-gyric crises and remarks thereon, he thought that much could be said on both sides.

Dr. Maudsley said he agreed with Dr. Bostock that certain oculo-gyric crises were hysterical, but it was not so in the vast majority. He cited a case of pseudopapillœdema which had been diagnosed by an ophthalmologist as papillœdema.

Dr. Brearley said that he was sure they were all interested in Dr. Campbell's remarks on papillœdema. The examination of visual fields were of the utmost importance and of great benefit to the neurologist. In regard to Dr. Maudsley's patient with pseudopapillœdema, as a rule true papillœdema was perfectly definite. It was extraordinary what a degree of papillœdema could be associated with normal vision. It was Dr. Lockhart Gibson who had first drawn attention to papillœdema associated with acute lead poisoning, and great praise was due to him. Dr. Maudsley had asked for the boundary line between the physiological and pathological colour of the discs. Dr. Brearley did not think it possible to decide. It was impossible to lay down any definite rule for this, as the colour of the discs differed so widely within normal limits in different patients.

Cataract.

Dr. Guy Antill Pockley (Sydney) read a paper on the prophylactic and operative treatment of early cataract. He confined his remarks to the types of cataract met with in middle and late life. For the purposes of discussion he divided prophylactic treatment into removal of the cause of cataract and local treatment other than surgical. Among the causes of cataract were such constitutional diseases as diabetes, various endocrine imbalances, chronic nephritis, autointoxication of various kinds. Cataract was also associated with the use of such drugs as ergot and naphthalin *et cetera*. It also resulted from local trauma to the lens. In the latter group the largest number of cases was associated with uncorrected ametropia or the wearing of incorrect glasses for a refractive error. Excessive ultra-violet light, heat, X rays and local intraocular circulatory disturbances could produce cataract. Another cause which he held to be operative was the unnecessary wearing of coloured glasses. When a glass was used which cut off the light, the pupil was left more dilated and more sluggish in its movements. This would lessen the rate of removal of aqueous fluid with comparative stasis of that fluid. The lens was entirely dependent on aqueous fluid

for its nutrition and if its rate of flow was diminished, nutrition must suffer. In increased illumination it was reasonable to assume increased metabolism in the eye and Nature dealt with this by producing miosis in bright light and consequently more rapid circulation of aqueous fluid. In regard to local treatment other than surgical, such forms of treatment were without exception empirical. Nothing had been discovered which could hope to clarify and restore to its original cellular condition broken down epithelial cells and coagulated lens protein. Applications of resorcin, dionin, subconjunctival injections *et cetera* had been acclaimed, but he had never seen any evidence of improvement associated with their use. Bathing with iodide solutions had some value. It also had a psychological effect on the patient, the fact that something was being done for him helped to remove the depression so common when cataract was diagnosed. He gave iodide lotion with pilocarpine drops. Finally Dr. Pockley dealt with surgical treatment. He described the method of preliminary iridectomy followed by extraction at a later date which he practised. The average time which elapsed between the iridectomy and the extraction, was seven weeks and three days.

Dr. R. H. Jones (Sydney) said that he was in agreement with Dr. Pockley and would not offer any criticism on the major portion of his paper, but he disagreed with Dr. Pockley in regard to one statement, that dark glasses raised the tension of the eyes by dilating the pupils. He did not see any reason to believe that the size of the pupils varied with dark glasses. He had used iodide solution for cataract, but was unable to say that he had ever seen any improvement from its use. If deficient thyreoid secretion was the cause of cataract, he would like to hear what effect the administration of thyreoid extract had and whether cataract had been produced experimentally by the production of thyreoid deficiency.

Dr. J. C. Douglas (Ballarat) said that the slit lamp and corneal microscope had revolutionized the study of early cataract. He was a believer in the medical treatment of early cataract. His treatment consisted in the correction of errors of refraction and errors of metabolism, including autointoxication, organo-therapy and chemicotherapy. The first line of treatment included the prescription of distance and near glasses with Crookes's lenses for outdoors. The lens of early cataract was often more myopic or less hypermetropic than formerly. He gave 0·015 milligramme of green iodide of mercury to stimulate excretion as well as to promote absorption of the products of oxidation from the ocular structures. The elimination of salt and the inclusion of much nitrogenous food and water formed a dietary suitable for hypertension. Sources of focal sepsis were investigated and corrected, especially in the teeth. Organo-therapy included the hypodermic injection of lens-antigen and the prescription of pluriglandular tablets. He had tried lens-antigen in six patients in conjunction with other treatment. In three patients definite improvement occurred, but in three none was noted. In the first patient the vision of the right eye improved from $^{6}/_{18}$ to $^{6}/_{9}$ and of the left eye from $^{6}/_{12}$ to $^{6}/_{9}$, while the multiple fine opacities all round the lens became distinctly fewer, smaller and lighter. In the second patient vision of the right eye improved from $^{6}/_{9}$ to $^{6}/_{6}$ and of the left eye from $^{6}/_{9}$ (three letters) to $^{6}/_{6}$ (six letters). There seemed to be an improvement in the opacities as shown by the series of diagrams made. In the third patient the vision of the right eye improved from $^{6}/_{9}$ to $^{6}/_{6}$ and of the left eye from $^{6}/_{18}$ to $^{6}/_{12}$ without any definite change in the lens opacities; it was a more advanced condition. In this patient pain and swelling in one knee joint appeared apparently as a reaction to the injections. In the remaining three patients there was less improvement. All expressed themselves as having clearer vision and feeling better after the injection. Objections to the lens-antigen treatment were the expense of the drug and the number of attendances necessary, involving also expense and trouble to the patient. Errors of endocrine gland balance as mentioned by Kirkpatrick had induced him to prescribe pluri-glandular tablets for the majority of patients with early cataract, with apparent local and general benefit.

Many writers asserted that early cataract was a hydrolysis or denaturation of lens fibres. Acting on this Dr. Douglas had been using for some years drops of calcium chloride, sodium iodide and distilled water in order to dehydrate the lens substance. This treatment resulted in improvement of vision. In some patients the opacities became quite invisible. All the patients had multi-glandular tablets and general treatment as well. James McKenzie had stressed the importance of studying the beginnings of disease. They should study cataract in its earliest stages and experiment with every therapeutic suggestion offered. In that way another brilliant chaper might be added to the book of preventive medicine.

Dr. J. Lockhart Gibson (Brisbane) said that he was an unbeliever that cataract could be influenced by most of the things that had been mentioned, except by attending to general health. He did not think cataract was a disease, but more a manifestation of senility. He thought that they would all get cataract if they lived long enough. In nuclear cataract he always did an upward iridectomy. He seldom ordered tinted glasses and thought a correct refraction was all that was necessary.

Dr. E. Temple Smith (Sydney) did not agree that tinted glasses caused cataract. He held that dilated pupils did not alter the flow of aqueous fluid. He had never had any result from the use of iodide solutions. He agreed with Dr. Douglas in trying to modify the cataract by metabolic means.

Dr. H. F. Shorney (Adelaide) said that he had used calcium iodide in early cataract for some years, but only remembered one patient who had derived any benefit. He thought that the patient should be informed that the drops might arrest the cataract, but never that they would cure it. The psychological effect of this statement was of great benefit to the patient. In regard to a patient mentioned by Dr. Pockley, who had a cerebral hæmorrhage whilst lying in bed soon after cataract extraction, he bled his patients whose blood pressure was above 180, either at the time of operation or an hour before and by this means he thought the danger of expulsive hæmorrhage was minimized. He regarded dark glasses as unnecessary, except in neurotic patients. Having recently read an account of cataract caused by exposure to X rays and radium, he thought the production of cataract should be borne in mind when patients had to be treated by these agencies.

Dr. H. V. Coverdale (Auckland) said that Dr. Pockley appeared to attach importance to the amount of flow of aqueous fluid in the ætiology of cataract. The metabolism of the lens was exceedingly small and he thought that the more important factor was the quality rather than the quantity of the aqueous fluid. With regard to coloured glasses, it was known that rays of short and long wave lengths could cause cataract. The former could be almost eliminated by ordinary glasses and the advantages derived from tinted glasses were probably subjective. He did not think that medical treatment was worth while for any cataract except that due to diabetes.

Dr. R. G. Waddy (Sydney) said that he thought the slit lamp should be of use for the diagnosis of early cataract, as the changes must be gross before they are able to recognize them with the *loupe*. If there was any hope of treating cataract, it must be in the early stages. He would like Dr. Pockley to give a detailed account of his maturation operation.

Dr. F. Abbey-Wiesener (Sydney) observed that no one in the room was wearing dark glasses, though there were several with glasses. This he considered spoke for itself in regard to dark glasses. The only tinted glasses of any use were those of Crookes. He came to this conclusion after having tested various types of coloured glasses in conjunction with Professor Vonwiller, of the Physics Department of the University. Crookes's glasses had been evolved to protect the workers in the glass industry and in this they were effective, as they prevented the workers from the infra-red rays which had been proved to be the cause of glass blowers' cataract.

Dr. R. Thompson (Brisbane) said that he approached immature cataract with a certain amount of temerity. He had performed the intracapsular operation of Smith, but

had not the same results in Australia as he had had in India and had given it up. He was pleased to hear of the results Dr. Douglas had got in treating cataract medically, but had not got the same results himself, although he had tried it. He was in agreement with the preceding speakers in regard to dark glasses.

Dr. D. D. Paton (Perth) said that as Dr. Pockley had cited coloured glasses as a possible cause of cataract, he was prepared to look carefully to see whether he found it the same; he did not believe that coloured glasses caused cataract. Dr. Pockley's method of maturation of immature cataract was of interest. He wondered whether the vitreous body became more fluid after this maturation; he wished to know whether Dr. Pockley used capsule forceps. He agreed with Dr. Pockley that they should be known as ophthalmic physicians rather than ophthalmic surgeons. He had used iodides of calcium and magnesium and had got improvement in vision. He had never noticed any alteration or disturbance of the striæ in the lens. He did not believe in the administration of lens-antigen as the results claimed were not substantiated and there was a danger of anaphylaxis. Lens-antigen depended on the crystalline substance to be effective. It was unstable. In regard to Dr. Douglas's suggestion that the salt intake should be diminished, he thought that salt did affect certain formations of eye disease, but had never thought of it in connexion with cataract. In early cataract he reduced the carbohydrate intake. Cataract was an inherited disease and any prophylactic treatment was rendered difficult because of this. He thought that the thyreoid might have some influence.

Dr. F. G. Baur (Sydney) said that he would like to emphasize the part refractive errors played in the production of cataract. He considered an accurate estimation of errors of refraction and prescription of glasses very important in the prevention of cataract which he thought was frequently due to the strain on the lens by the ciliary muscle in the presence of astigmatism.

Dr. Colin C. Ross (Sydney) said that he would like the views of those present in regard to whether patients who had some definite small lens opacities, should be told that they had cataract. He felt that the psychological fact of informing them they had cataract did harm. At the same time there was a danger if a patient consulted another ophthalmologist and was told he had had cataract, that the impression would be created that the first man had missed seeing the opacities.

In reply Dr. Pockley said that it appeared that the members did not accept his suggestion that coloured glasses could cause cataract. He had brought forward this as a clinical fact thought to be observed. The explanation was offered as a purely hypothetical one. The members appeared to be practically unanimously opposed to the wearing of coloured glasses in ordinary health. He expressed the hope that they would observe whether or not their own clinical findings in the future pointed to coloured glasses acting as a cause of lens opacities. As Dr. Coverdale had pointed out, ordinary optical glass cut out all the ultra-violet rays. Those which penetrated, only penetrated for two or three millimetres. They could not reach the eye. There was the anomalous position of the spectacle makers claiming that their respective coloured lenses were necessary to protect the eyes from ultra-violet light, while those interested in light therapy had been unsuccessful in finding a glass through which all ultra-violet rays could pass for purposes of treatment by sunlight therapy in glass enclosed spaces which would protect patients from adverse atmospheric conditions. In reply to Dr. Temple Smith he said that no improvement was claimed for iodides. It inhibited lens changes. The eye absorbed many drugs much better than most epithelium covered surfaces. He could not agree with Dr. Gibson that all cataracts were natural phenomena like grey hair. He regarded diabetic and myxœdematous cataracts as instances of disease. He referred Dr. Abbey-Wiesener to Leonard Mitchell's report on the influence of various coloured glasses on light. He did not use any drug treatment preliminary to operation beyond the local instillation of cocaine. Opacities in the lower part of the lens frequently were attributed to more light exposure. He suggested as an alternative explanation the formation of fluid within the capsule proved to exist in association with development of some forms of cataract. This, even in very small quantities, would tend to fall to the lower part of the lens as the result of gravity, a point possibly worth investigating. He had not practised bleeding the patient before operation for high blood pressure. He had not observed any signs of hydræmia of the vitreous body as a result of the complete evacuation of the aqueous fluid. For rapid and certain maturation the point considered to be essential was complete evacuation of the aqueous fluid, particularly from the retrolental space. This took time, as the aqueous fluid had to come forward through the supporting ligament of the lens into the anterior chamber. It probably took three minutes to evacuate this. Before massage the lens should be cushioned on the vitreous body, so that there was no tendency for it to tilt. Massage should be applied radially from the centre of the cornea rather than circularly, in order to avoid squeezing the iris between the cornea and the lens. If done radially, the iris receded before the point of the iris repositor. After cocaine was administered, the lids should be kept closed till the operation was begun, as the corneal epithelium might be rubbed off. Of his series of two hundred consecutive cataracts of all sorts, mostly immature, 25% had been needled. When the vision was $^{6}/_{9}$ or better, needling was not undertaken. He gave the patient or his friends information that there was an abnormality when lens opacities were present. His method of imparting information varied with the psychology of the patient.

Bone-Free Skiagrams of the Globe of the Eye.

Dr. H. F. Shorney (Adelaide) read a paper in which he described the method of Vogt of taking radiographs of the anterior half of the globe without bone shadows. He said that there were three indications for taking these photographs. The first was the type of case in which an intraocular foreign body was suspected and when after the usual skiagram had been taken, a foreign body was not found or was not seen with certainty. The second was the type of case in which the suspected foreign body was of such delicate material that in the developed photograph taken by the intense rays necessary for a photograph of the skull, it created no shadow at all. The third was the type of case in which there was a small foreign body which had been diagnosed but not localized and was possibly of a semitransparent nature. The method was limited in that only the anterior portion of the globe could be photographed. It was possible to go back as far as the equator or a little beyond that. Dr. Shorney then gave details of the application of the lead foil containing two films to the eye ball with moderate pressure, of the position of the patient and of the tube and dosage. Five exposures were made with the films facing in the following five directions: straight ahead, in a temporal direction, upwards, in a nasal direction and downwards. In this way the maximum amount of the globe could be photographed. Dr. Shorney then explained the way in which localization was carried out by the application of small metal fish hooks with the tube in the frontal position.

Dr. Guy A. Pockley (Sydney) said that he would like to congratulate Dr. Shorney on his courage in implanting a foreign body in his own eye in order to get the photographs shown, and thanked him for his paper.

Dr. J. Lockhart Gibson (Brisbane) also expressed his thanks to Dr. Shorney for the paper. He quoted a case he had at Lemnos during the war, in which a foreign body was not seen in the X ray photograph. By using Dr. Shorney's method without the bone shadows of the orbit, he thought it undoubtedly would have been seen.

The Treatment of Aucte Glaucoma.

Dr. Howard V. Coverdale (Auckland) read a paper on the treatment of acute glaucoma based on the records of twenty-four cases. In twenty instances there had been a history of inflammation and pain from one day to two weeks in duration, but no history of ætiological significance. In the remaining four there had been history and evidence of previous chronic glaucoma. Dr. Coverdale prefaced his remarks on treatment by reference to the slit

lamp and to the occurrence of keratic precipitates in glaucoma. His observations supported the suggestion of Greeves that the presence of *keratitis punctata* in a glaucomatous eye might not necessarily mean that the glaucoma was secondary. It was possible in some cases at least that an irido-cyclitis might occur as a result of a prolonged glaucomatous condition. In many of his cases it had been thought that the glaucoma was secondary to an active irido-cyclitis and treatment had been directed first to reducing tension and inflammation by medical means so that a cure might possibly result and that trephining could be performed. In most instances some reduction of tension had been apparent and a more detailed examination of the eye had become possible. If the tension had returned to normal and if signs suggestive of irido-cyclitis were present, homatropine had been substituted for eserine until the tension showed signs of rising again. By these means in a series of twenty cases of glaucoma of doubtful origin four patients had been cured with eserine and two with eserine and homatropine. Of the remaining eighteen patients of the whole series of twenty-four, eleven had been submitted to the operation of trephining. From the operative point of view the result of trephining had been good in nine instances; from the point of view of restoration of vision the results had been good or satisfactory in eight. Dr. Coverdale's conclusions were that the results of trephining for the relief of acute glaucoma were good and that the operation might be recommended whenever it was technically possible. Excepting when chronic glaucoma had been a precursor, medical treatment might safely be persisted with for several days, but when chronic glaucoma had been present immediate operation was advisable. The presence of keratic precipitates or weak posterior synechiæ need not contraindicate operation or presage injurious post-operative inflammation. If a white star-shaped figure were present on the lens capsule, the prognosis was less favourable.

Dr. J. Lockhart Gibson (Brisbane) said that there were three attitudes, the physical, the physiological and one between the two. He had upheld the secretory view of acute glaucoma. Chronic glaucoma had a different ætiology. He cited the work of Leonard Hill and also referred to the recent findings of Duke Elder. The latter had found that the intraocular fluid was a dialysate and stated that the blood pressure in the intraocular capillaries was thirty millimetres of mercury higher than the intraocular pressure. The truth of this contention had been challenged. Dr. Lockhart Gibson supported the view that a toxin induced increased tension in the capillaries and an increased flow into the vitreous chamber. This resulted in a stimulation of the secreting cells. He also quoted from an article by Woollard. Interchange of fluid between the closed spaces of the body was controlled by cellular barriers which possessed selective absorption. Dr. Lockhart Gibson suggested that a chemical, biochemical or bacterial irritant acted on the secreting cells of the capillaries of the ciliary body and induced secretion of the intraocular fluids into the vitreous chamber. The communication between the posterior and the anterior chambers was blocked. He traced the events of acute glaucoma from this beginning. He claimed that an incision into the angle of the anterior chamber, through the sclerotic, followed by a basal iridectomy cured the acute glaucoma. The increased tension did not recur even when other causes gave rise to glaucoma in the other eye. He also spoke of a form of glaucoma due to arterio-sclerosis and endarteritis. The blood was banked up in the arterioles, because the veins were compressed and obstructed by hardened arteries. The increased flow from the capillaries into the vitreous chamber was the result of raised blood pressure. This variety of glaucoma was not helped by operation. He used salicylate of soda and eserine locally and gave morphine to ease the pain and to lessen the secretory activity of the ciliary gland.

Dr. E. Temple Smith (Sydney) said he would confine his remarks to the difficulty of diagnosing between primary and secondary glaucoma. This diagnosis depended on the right conception of the pathology on which also the treatment depended. When he found punctate keratitis and posterior synechiæ he regarded it as secondary glaucoma and had no hesitation in using atropine. In severe glaucoma he performed a de Wecker's section, which was really an uncompleted cataract section, a bridge of tissue being left by not completing the section. In mild conditions he merely did a paracentesis. He regarded the treatment mentioned by Dr. Coverdale of eserine and subsequently homatropine unsound and the results quoted would appear to confirm this. He thought that a posterior scleral puncture was a most valuable procedure. The great trouble with these conditions was in the diagnosis.

Dr. Guy A. Pockley (Sydney) said that he had been taught that the correct treatment to carry out in acute congested glaucoma was immediate iridectomy. He was very glad to know that Dr. Coverdale believed in preliminary treatment. As a result of doing immediate iridectomy he had had two tragedies from expulsive hæmorrhage. He had given up immediate operation. He used 0·25% eserine and adrenalin drops and leeches. In nearly every patient treated that way the tension returned to normal within twenty-four hours, but this rose again within twenty-four to forty-eight hours. He was not satisfied with the results of trephinings in the presence of congestion and he did not think that the disadvantages of waiting for twenty-four hours were to be compared with the risk of hæmorrhage in operating at once. In one instance by waiting he had found a melanotic carcinoma, which modified treatment. He operated with the patient under a general anæsthetic and regarded ether a bad anæsthetic to use owing to the congestion it produced; he used chloroform.

Dr. H. F. Shorney (Adelaide) suggested that a post-orbital injection of a local anæsthetic might be valuable, although he had never tried it. In ring synechiæ he thought an iridectomy was indicated. Where the anterior chamber was deep it was rational to perform paracentesis, but he would not advise repeated paracenteses. He favoured posterior sclerotomy and in acute glaucoma preferred iridectomy, as he did not consider a trephine a satisfactory operation in this condition.

Dr. James Flynn (Sydney) mentioned an interesting case of glaucoma resulting from the instillation of a mydriatic. He had performed a posterior sclerotomy and iridectomy. On the following day acute glaucoma had arisen in the other eye. He trephined the second eye after preliminary treatment had been carried out. He believed in paracentesis and he pointed out to Dr. Shorney that only one paracentesis was necessary as the wound could be opened repeatedly.

Dr. A. P. Lawrence (Melbourne) had attended a woman, aged twenty-eight years, who had been admitted to hospital suffering from double acute glaucoma. She had been found to be suffering also from acute nephritis supervening on chronic nephritis. Both eyes recovered completely with conservative treatment. He thought that the administration of trional and hyoscine might enable operation to be performed without an anæsthetic.

Dr. R. H. Jones (Sydney) understood Dr. Coverdale to say that great pain was not an outstanding feature in acute glaucoma. His experience and Dr. Coverdale's were not in accord. In regard to operation, he had noted that the operation of irido-inclisis was now being written about. This operation had been performed by the late Odillo Maher years previously.

Dr. R. G. Waddy (Sydney) said after hearing Dr. Coverdale's paper that he would like to ask if they were to expect *keratitis punctata* in an ordinary acute glaucoma and if Dr. Coverdale found the *keratitis punctata* mentioned with the *loupe* or with the slit lamp.

Dr. D. D. Paton (Perth) said that he did not agree with Dr. Temple Smith that eserine was absolutely contra-indicated in doubtful cases. He had given up trephining in acute glaucoma, but did an iridectomy. He disagreed with Dr. Pockley that ether for the anæsthetic was contra-indicated, as he did not think the modern method of intratracheal administration of ether caused any disability.

In reply Dr. Coverdale said that he was very interested to hear the many pertinent comments and criticisms of his paper. His conclusions should be regarded as only tenta-

tive. He agreed with Dr. Lockhart Gibson that acute glaucoma appeared to belong at times to a clinical group separate from those following chronic glaucoma. Possibly vascular thrombosis was important in these conditions. Regarding the secretory function of capillary endothelium, he had not seen anything clinically to disprove the view of Duke Elder. He was very interested in Dr. Temple Smith's remarks. Many of his patients had been treated as though the glaucoma were secondary in origin. If the clinical signs he had mentioned were accorded their usual value, there would remain only two patients in his series whose glaucoma could be considered primary. In regard to operation he agreed that several procedures such as sclerotomy might be very successful. The difficulty lay in knowing which operation to choose in a given case. He agreed with Dr. Pockley that owing to the difficulty of observation there was much guesswork in examining glaucomatous eyes. A sarcoma, a dislocated lens or a vitreous hæmorrhage might easily escape notice. After intensive medical treatment differential diagnosis was usually easier. He did not agree with Dr. Shorney that paracentesis was very valuable, even in mild or early glaucoma. It was, he thought, occasionally of value as a temporary measure. Dr. Waddy had understood him correctly. The keratic precipitates were not visible with the *loupe* until the tension had fallen. In reply to Dr. Paton he said that he had trephined inflamed eyes without great difficulty, but an assistant was necessary in order to swab with adrenalin. He had recommended immediate operation in cases following chronic glaucoma because the visual results were so bad. He found it usually impossible to examine the field satisfactorily when the eye was inflamed, because the patient was usually ill and in pain and the vision was rarely more than the perception of hand movements. If these conclusions were right, differential diagnosis between primary and secondary acute glaucoma became a matter of even greater difficulty. History was important and, of course, fibrinous adhesions suggested a definite inflammatory cause. Further and more accurate slit lamp observations than his would do much to help them reach a better understanding of one of the most difficult problems in ophthalmology.

Scotometry in the Diagnosis of Chronic Glaucoma.

Dr. E. C. Temple Smith (Sydney) read a paper on scotometry in the diagnosis of chronic glaucoma. He referred first of all to the small object scotometer of Bjerrum. This method had entailed a large black screen on a well lighted wall over the surface of which was moved a one or two millimetre white object on the end of a black stick. The patient had been placed at a distance of one metre from the screen. Though sound in principle, the method had not been popular because of its practical difficulty and the time taken in getting records. Priestley Smith had then suggested that the field should be explored circularly instead of radially and instead of moving the object, he had rotated the screen. Priestley Smith's scotometer had a disc whose diameter was a third of a metre; the patient was placed a third of a metre from the disc. The scotometer introduced by Elliot in 1916 combined the principle of Priestley Smith's circular method with Bjerrum's magnification of a small object at a metre distance. Priestley Smith's assertion that when no scotoma was found in the five, ten or twenty degree circle glaucoma could be excluded, remained a truism. Dr. Temple Smith said that he attached great importance to the findings of the scotometer and gave details of the histories of several patients to illustrate his remarks. He also showed the charts of the patients.

Dr. T. C. Ker (Bendigo), referring to the question asked by Dr. Temple Smith as to what should be done in glaucoma with diminishing fields in which no increased tension could be demonstrated, advised trephining, as in his experience, although this did not stop the continued diminution of the field, he thought that it was lessened by this means.

Dr. J. Lockhart Gibson (Brisbane) said that chronic glaucoma without increased tension was at times due to senile arterio-sclerotic changes due to altered metabolism; he advised small doses of thyreoid. He thought that the condition was very difficult to treat, as in his experience the patients went downhill whatever was done.

Dr. R. G. Waddy (Sydney) thanked Dr. Temple Smith for his very able exposition of the art of perimetry. The diagnosis of early glaucoma was very difficult and puzzling. He was always pleased to have Dr. Temple Smith's opinion as to the condition of the field. He asked if there was a model of the machine to be had locally and whether there was a self-registering type. He also wished to know to what extent the peripheral field could be charted in comparison with the ordinary perimeter.

Dr. H. F. Shorney (Adelaide) mentioned in regard to the field that his method was to watch the notch in the field and if this was approaching fixation point, he regarded it as an indication for operation.

Dr. D. D. Paton (Perth) said that Elliot's scotometer was a valuable instrument. In glaucoma in which no increased tension could be demonstrated, he had seen it suggested that the tension might be estimated two or three times a day and charted as the ordinary temperature chart was done; in this way increased tension might be discovered.

In reply to Dr. Gibson, Dr. Temple Smith did not think the large cupping of the discs could be accounted for by metabolic changes. Dr. Gibson had said that there was a self-recording machine in Brisbane, but Dr. Temple Smith did not know of a self-registering model. Dr. Temple Smith said that the peripheral field diminution began later than the enlargement of the blind spot and if he had to choose between a perimeter and a scotometer, he would unhesitatingly use the latter. He had had models of the machine he demonstrated made at the Royal North Shore Hospital.

The Treatment of Lachrymal Obstruction.

Dr. T. C. Ker (Bendigo) began his paper on the treatment of lachrymal obstruction by referring to a meeting in London in which reports of operations for chronic dacryocystitis by the making of an opening into the middle meatus of the nose were greeted with a chorus of disapproval. He said that he had not had satisfactory results with the only three remaining methods: syringing and probing, probing followed by the wearing of a style and excision of the lachrymal sac. He reported the performance on twenty patients of an operation consisting of the making of an opening between the inner sac wall and the nose. He removed a piece of the frontal process of the superior maxilla, passed a probe into the sac and made it bulge into the nose. He then destroyed the wall of the sac with an electric cautery. The operation was performed under local anæsthesia. He had been able to get into touch with fifteen patients out of the twenty. Three had been operated on over six years, seven over three years, two over two years and three about a year. One patient still suffered from discharge and watering of the eye, the eye of another only watered in the wind and the eye of a third watered slightly, but not more than the other eye. All the others had stated that they were free from discharge and watering. He had found that no after treatment was necessary except the sniffing into the nostril of an oily solution every day for two or three weeks.

Dr. H. F. Shorney (Adelaide) wished to ask Dr. Ker what type of sac he considered suitable for the operation, whether the sac had a very thin wall or the wall was of a thick variety. There had been much talk about this operation in Vienna, but they did not seem to have come to any conclusion about it. Frequently the object of removal of the sac was to get rid of pneumococci and he wished to know whether pneumococci disappeared after this operation. Another point that he wished to bring forward was that the ethmoidal cells sometimes extended forward and as Dr. Ker described the operation, these would be opened, which he did not think advisable.

Dr. E. Temple Smith (Sydney) said that he considered the old operation for the removal of a sac unsound pathologically and he was interested to hear Dr. Ker's results. He thought the principle of the operation a good one.

Dr. J. C. Douglas (Ballarat) said that he was very interested in Dr. Ker's paper. He described a treatment

with chromic acid and hydrogen peroxide for which good results had been claimed; he had been disappointed in it.

Dr. D. D. Paton (Perth) said that X ray photographs of the sac showed various anatomical obstructions and sacculation of the sac. The objection he saw to Dr. Ker's method was that if the opening into the nose was in the upper part of the sac, a trap was left below for the collection of pus and mucus.

Dr. Ker was unable to answer Dr. Shorney's question owing to the small number of these operations he had performed. He was interested to hear of Dr. Douglas's experience with chromic acid which at one time he was thinking of trying.

Central Tarsorrhaphy.

Dr. J. C. Douglas (Ballarat) read a paper on central tarsorrhaphy. He enumerated the conditions in which the operation might be undertaken: acute septic corneal ulcers in their later stages, chronic corneal ulcers which were clean looking, neuropathic types of corneal ulcer or neuropathic keratitis, staphyloma of the cornea, burns of the skin of the eyelids in which cicatricial deformity was to be feared or impending, in plastic operations on the lids to secure immobility, cicatricial ectropion, paralytic and senile ectropion, lagophthalmos, exophthalmos due to exophthalmic goitre, proptosis of the eyeball from causes such as aneurysm or tumour, anæsthesia with paralysis of the fifth nerve as a preliminary to excision of the Gasserion ganglion, paralysis of the seventh nerve. Dr. Douglas then described the technique of the operation and said that it was important to keep the eyes bandaged for some days until union was firm. A chink was left at either end of the palpebral fissure and the conjunctival sac could be washed out through these openings. In some conditions the union was allowed to remain permanently. Dr. Douglas concluded by giving representative case histories in detail.

Dr. R. G. Waddy (Sydney) said that he favoured tarsorrhaphy to the side rather than the centre, as vision was not interfered with to such an extent. He thought that the ectropion shown on the screen by Dr. Douglas would have been very much better with a plastic operation.

Dr. E. Temple Smith (Sydney) said that he thought some of the indications for this operation mentioned by Dr. Douglas quite inapplicable. It was a very valuable procedure in neuro-paralytic conditions.

Dr. H. F. Shorney (Adelaide) said that in one patient mentioned by Dr. Douglas for whom he had done tarsorrhaphy, when there was an imminence of a staphyloma bursting, he would have taken the eye out.

Dr. D. D. Paton (Perth) said that in the patient with ectropion shown by Dr. Douglas, he had noticed that a lateral tarsorrhaphy had been done and he felt that the stitches in a central tarsorrhaphy would have given way. He supported its use for neuro-paralytic conditions, but some of the corneal conditions mentioned by Dr. Douglas he thought would have been more satisfactorily treated by pulling over a conjunctival flap.

Dr. Douglas said that he quite appreciated the criticism. With regard to the conjunctival flap mentioned by Dr. Paton, he did not think it would have lasted long enough. Referring to the remarks made by Dr. Waddy on the case of senile ectropion, he agreed that a plastic operation was generally indicated, but this patient was so old that he thought tarsorrhaphy more suitable. He realized that this operation was only advisable in selected conditions.

SECTION OF OTO-RHINO-LARYNGOLOGY.

President's Address.

Dr. R. Graham Brown (Brisbane) in his address as President of the Section of Oto-Rhino-Laryngology began by discussing the value of criticism and the place which it ought to occupy in medical practice and discussion. He discussed the recent advances made in the application of radium to malignant disease. In reference to malignant disease of the tongue, pharynx, palate *et cetera*, he thought that throat surgeons should undertake the implantation of radium needles required for treatment. They possessed the necessary dexterity and were used to manipulating mirrors and instruments in the bucco-pharynges of their patients. He was also of the opinion that in any one institution the patients with malignant disease of the throat, mouth, tongue *et cetera* should be placed under the care of one man, for in this way it would be more likely that a specialist would develope. In regard to cancer of the larynx he thought that before long no larynges would be excised, but he did not think that at the present time radium was wholly to be preferred. He thought that before long throat surgeons would be forced to assume the responsibility of treating patients with malignant disease of the œsophagus. In regard to local and synergistic anæsthesia he said that it was to be preferred whenever possible to general anæsthesia. They would find that as a general rule the man who practised local anæsthesia, had an operative technique that was more refined than that of a man who operated under general anæsthesia alone. In conclusion Dr. Graham Brown put forward the view that ear, nose and throat surgeons should regard operations on the thyreoid and operations for removal of the larynx as their sphere. They should not be debarred from removing tumours of the pituitary body through the nose and since they were used to operating on the mastoid bones, they would before long be attacking tumours of the cerebello-pontine angle.

Dr. R. S. Godsall (Sydney) said that Dr. Graham Brown had shown them the path to tread with regard to establishing their ability to do a certain amount of general surgery. He had often had to remove cancer of the larynx after a general surgeon had opened the larynx. He had also seen partial laryngectomy performed unsuccessfully by general surgeons; he thought that the operation should be done by laryngologists. The younger men especially should try to become efficient in general surgery. Radium workers should secure the aid of ear, nose and throat surgeons. Some patients had been treated with radium before a laryngeal examination had been made, but radium was being applied in Sydney under the direction of oto-laryngologists who made the diagnosis and noted progress.

Dr. G. E. Scantlebury (Melbourne) said that in Melbourne the physicians and surgeons frequently consulted oto-laryngologists; for example, four patients suffering from pituitary tumours had first been seen by a physician or a surgeon and had been referred to him for treatment. Radium had been applied by oto-laryngologists in close cooperation with general surgeons, physicians and radiologists during the past six years.

Dr. H. S. Marsh (Sydney) was interested in local anæsthesia in ear, nose and throat conditions; he had not used it much. Local anæsthesia was trying for the operator, but with experience he found the technique easier. He had used it only in certain tonsil operations. Dr. Corlette was the exponent in Sydney; he had assisted Dr. Corlette in performing a laryngectomy with complete success. Radium had not long been in use in Sydney; at the Sydney Hospital the needles were of too high a dosage; he had used radium in only one case of carcinoma of the tonsil and the result so far had been good.

Dr. E. Culpin (Brisbane) said that the disciples of local anæsthesia sometimes found the temperament of the patient a difficulty. For tonsils he preferred a general anæsthetic; a local anæsthetic was a great strain on both the doctor and the patient; the chief point in using this method was the preliminary treatment which required a properly trained staff. He had seen Dr. Graham Brown perform laryngectomy. It had been easy for all, including the patient. He had controlled the patient's pulse right through and it was steady and strong. The patient had been well and happy on the following day. A better result could not have been obtained with general anæsthesia. One operation which was specially suitable for local anæsthesia, was the radical antrum operation. There was no hæmorrhage and the operation was far more efficiently performed, though the patient sometimes felt a little pain in regions which were difficult to anæsthetize. He had used radium in Brisbane during the past six months, but so far had not had the opportunity of using it for cancer of the larynx. He had used it for a malignant ulcer at the

base of the tongue and glosso-epiglottic region in which the cervical glands had been previously resected. He had performed a preliminary tracheotomy and there had been a reaction in the epiglottis afterwards which settled down later. Later, as there had been further signs at the base of the tongue, he had introduced radium needles through a trochar and cannula, by which means the points and eyes of the needles were not damaged, as would have been the case had forceps been used.

Dr. H. M. Jay (Adelaide) remarked that they seemed to be working further up and further down from their region and asked where they were going to stop. Aural surgeons were operating on brain abscesses and were doing bronchoscopy. They were covering much ground; he regretted that no mention had been made of the use of diathermy for laryngeal affections, as this method was advancing side by side with radium.

Dr. A. J. Cahill (Canberra) agreed with Dr. Graham Brown's remarks about local anæsthesia, especially the use of nerve blocking with cocaine paste which gave perfect anæsthesia for operations on the septum, ethmoids and sphenoids. There were no bad after effects. He regretted that diathermy had not been mentioned for the removal of the tonsils; he had used this method after previously anæsthetizing with 10% cocaine. Only three or four sittings were necessary; there was no loss of blood, no loss of work and no hospital fees, only the doctor's fees.

Dr. W. Crosse (Brisbane) said that the local anæsthesia had been used at one time in Brisbane Hospital for the removal of tonsils, but this operation was being performed with intratracheal general anæsthesia. The gagging was the chief difficulty in this operation under local anæsthesia. He had had his own antrum operated upon by Dr. Culpin under local anæsthetic and morphine. He had not felt anything at all.

Dr. Graham Brown in reply stated that he was glad that the Melbourne practitioners were consulting the oto-laryngologists for ear, nose and throat conditions in which radium was used. He said that Cushing would not allow an otologist to touch the pituitary. Cushing approached the pituitary from under the upper lip and took three-quarters of an hour while otologists completed the operation in ten minutes. In regard to diathermy, he wished to point out that men like Douglas Harmer and Cumberbach who were the fathers of diathermy in Great Britain, had given it up in cancer of the tongue. Harmer was always ready to replace old methods by new ones, if he was convinced they were better. The secret of successful local anæsthesia was in the preoperative treatment; he placed all his patients in the same hospital with the same well-trained staff under ideal conditions. The patient should be in the right state of mind when he entered the theatre; otherwise he was sent back; certain ideas, such as the risk of hæmorrhage in adults, had to be abolished to produce this state of mind. A patient should be drowsy so that all reflexes and sensibilities were diminished. He agreed that tonsillectomy under local anæsthesia was sometimes difficult; he thought it a mistake to operate with the patient in the sitting position and preferred the lying position, though it was a little more difficult and required an assistant and a sucker.

The X Ray Diagnosis of Mastoiditis.

Dr. Donald Smith (Perth) read a paper in which he discussed the X ray diagnosis of mastoiditis and reported the results of 220 examinations. He tried to estimate the assistance that had been rendered to the clinicians by this means. Both mastoids had been normal in 42 of the 220 patients examined; in 44 both mastoids had been affected and in 134 only one mastoid. Subacute mastoiditis had been present in 16, in three of these the process had ended in suppuration. In these mastoids no bony changes had been seen, but only patchy dulness. Acute suppurative mastoiditis had been diagnosed in 75 instances. This diagnosis had been made when no cell contained any air and the mastoid region was uniformly dull. Chronic sclerotic mastoiditis had been present in 132 ears; in this group had been placed all those ears in which otosclerosis had caused obliteration of the mastoid cell pattern. Thus 223 of the 440 ears had manifested morbid change. The type of mastoid likely to provide a path of infection into the mastoid was the cellular or pneumatic type and 81% of suspected mastoids had been of this type. All the mastoids described as non-pneumatic had contained evidence of old standing infection. It appeared more than probable that all non-cellular mastoid processes were the result of bacterial infection. In 102 instances the mastoids had been approximately symmetrical, in 17 definite asymmetry had been present and in 101 instances the anatomy had been unrecognizable on account of pathological changes. In regard to the accuracy of the X ray findings, the results had been confirmed by operation in 102 instances and the error had been 4·9%; the findings had been confirmed by clinical results in 118 instances and the error had been 1·7%. This compared more than favourably with any other means of diagnosis. The final conclusion was that X ray investigation was definitely helpful in 38% of cases, worth while in 59% and misleading only in 3%.

Dr. H. J. Gray (Perth) advocated having an X ray examination carried out of every patient before mastoidectomy, particularly to determine the position of the lateral sinus and the extent and position of the mastoid cells. If some cells were involved, slight dulness would be seen in the skiagram; it was safe to watch the patient. When all the cells appeared to be cloudy, osteitis was present and immediate operation was essential. Similarly, no delay was permissible if there was destruction of the bony intercellular walls. He was prepared to continue non-operative treatment of suppurative *otitis media* with persistent pain, provided that the skiagram did not reveal involvement of the mastoid cells. At times the cells were seen to be involved to the extent of even cholesteatomatous degeneration without any other sign or symptom of mastoiditis. In chronic conditions gross sclerosis was common. The skiagram often provided information concerning the lateral sinus and *dura mater* in these patients. The skiagrams were very useful in enabling the surgeon to plan operations beforehand and in determining the condition in compensation claims.

Dr. H. M. Jay (Adelaide) said that stereoscopic skiagrams were the best for the mastoid area; he was certain that if every patient with acute suppurative *otitis media* were examined radiologically, cloudy swelling would be frequently found. In the hands of men inexperienced in radiography this would be a pitfall, because many patients recovered without a mastoid operation; the conditions which caused him most embarrassment were those in which the report of the radiographer was: Bony changes shown which might be due to old sclerosis or to active acute inflammation.

Dr. J. H. Shaw (Melbourne) appreciated Dr. Donald Smith's results, but criticized his statement about the conversion of the pneumatic type into the sclerotic and also the rarity of the latter. Cheatle's collection in London was evidence that the sclerotic type was not the result of infection.

Dr. A. H. Murphy (Brisbane) hoped that Dr. Donald Smith would continue with his X ray work on the mastoid process and suggested that he should photograph normal skulls to get more information about asymmetry. Dr. Smith had found that only 14% of the mastoid processes were asymmetrical; Schiller, of Vienna, had found that 50% were asymmetrical. He agreed with Dr. Smith's classification of mastoiditis. Cheatle considered that the sclerotic type was due to inflammation.

Dr. J. O'Sullivan (Melbourne) considered that some of the X ray pictures illustrating chronic mastoid disease presented the appearances of undeveloped mastoid processes. It was possible that inflammation had occurred earlier in life.

Dr. G. E. Scantlebury (Melbourne) claimed that Cheatle had found that 16% of the mastoid processes exhibited asymmetry; this corresponded to Dr. Smith's figures.

Dr. E. Culpin (Brisbane) said that Dr. Smith's figures for asymmetry should be doubled, as he had taken only half the total number. He would like to know what was

the skiagraphic effect of extra-mastoid œdema. He wished to know whether it was possible to distinguish an external *otitis* from mastoiditis on the skiagraphic appearances. He thought that sometimes the radiologist reported that mastoiditis was present when there was really only œdema over the mastoid.

Dr. A. J. Cahill (Canberra) said that when the diagnosis was in doubt he employed ionization with sodium salicylate which reduced the swelling of a furuncle within twenty-four hours, but had no effect on the œdema of mastoiditis.

Dr. R. Graham Brown (Brisbane) stated that the work presented was evidence of much patience and tenacity. This was the first attempt of its kind in Australia. X ray pictures of the mastoid process were very difficult to interpret; he preferred a stereoscopic picture. At times it was difficult to decide whether to operate or not; radiographic evidence often proved useful. The radiologist sometimes advised operation, but he had waited one or two days and the patient had improved and finally recovered. He asked whether Dr. Donald Smith thought that this recovery might possibly be due to the exposure to X rays; he himself thought it might be. He thought that opacity of the cells did not necessarily mean that an operation was necessary. The patients sometimes improved after paracentesis had been performed.

In reply Dr. Donald Smith pointed out that his figures for asymmetry were on a percentage basis and were not affected by the fact that he had used only one-half of the total number of patients. In regard to the length of time that sclerosis had been present, the skiagram gave no information; it merely disclosed its existence. It revealed the signs of actively advancing sepsis. Dense, blotchy islands indicated the presence of infection at some time. The series had been studied because in all the patients there had been suspicious ear symptoms. There had been a high percentage of the pneumatic type in the series; it appeared that the pneumatic ear was more likely to become infected. He maintained that the skiagrams could not lie; the interpreter was sometimes at fault. The skiagrams revealed the condition of the ear at the time the picture was taken and by the time operation was performed this condition might have altered. A series of skiagrams taken every day would give fuller information. Once all the cells had disappeared, they could not reform. When complete dulness of all the cells was seen in the skiagram, pus was always found at operation.

Dr. Gray said that radiography was of great value in the differential diagnosis between external *otitis* and mastoiditis. If the result of the X ray examination and the clinical findings left any doubt, it was best to wait. Very slight cloudiness of the cells was seen when recovery was likely to occur, but when there was much cloudiness of the cells, the inflammation had come to stay. If there was much dulness there was certainly suppuration. If there was very slight dulness only and the clinical condition was good, it was wise to wait.

Frontal Sinusitis.

Dr. H. Seaward Marsh (Sydney) read a paper on acute fulminating frontal sinusitis. He based his remarks on three cases which had come under his observation. In each instance an orbital abscess had been present and pus had been found under the periosteum of the forehead together with purulent ethmoiditis and complete disorganization of its cells and in two instances abscess of the brain. He laid emphasis on the necessity for radical treatment, including exploration and exenteration of the ethmoid. It was necessary to remember the probability of the occurrence of cerebral complications and the occurrence of a fit was an undoubted sign of abscess formation. He drew attention to the fact that in each of his three patients the antrum had escaped infection.

After reading his paper Dr. Marsh quoted a fourth case in which there was bulging of the eye forward five days after the onset of the trouble, and an orbital abscess had formed. He had performed a similar operation on the left frontal and left ethmoidal sinuses and had left the wound open. Five days later, by inspecting the interfrontal septum, he had been able to see that the right frontal sinus was also infected; he had performed a second operation on the right frontal and right ethmoidal sinuses. Later another complication had occurred, cellulitis of the whole of the scalp and the formation of four subperiosteal abscesses.

Dr. W. Kent Hughes (Melbourne) said that since hearing of Dr. Godsall's method of packing the frontal sinus and obliterating it he had employed this method. He did not often remove the floor of the sinus. When he had performed a radical ethmoid operation at the same time as the frontal operation and had enlarged the opening of the naso-frontal duct, he had generally found adhesions in the duct years later. He considered that complete obliteration was the ideal method. To do this, however, it was necessary to reach all the diverticula. The best way of removing the ethmoidal cells was by the external operation, although it was true that the first ten operations of this kind performed in England had been fatal. The explanation was that operation had been put off too long. The only intranasal treatment he had used was removal of the anterior end of the middle turbinal bone in treating frontal disease. If more were required, he performed the external operation.

Dr. R. S. Godsall (Sydney) stated that affections of this sort were sudden and catastrophic; in two months he had had two patients; in one of them the frontal and ethmoidal regions were rotten and there was pus in the organs. He had performed the operation in twenty minutes owing to the patient's bad condition and had left the whole wound wide open. It was better not to worry about the cosmetic effect at the time, but to save the patients' lives and to do plastic operation later. His patient had had fits two weeks later, which he regarded as a diagnostic point in frontal lobe abscess. He had therefore removed the posterior wall of the frontal sinus and had found pus in the frontal lobe which he had drained. The patient had recovered and ten weeks later he had performed a successful plastic operation. The second patient was a child of eight in whom an orbital abscess had formed following frontal and ethmoidal disease soon after an attack of scarlet fever. He had performed frontal and ethmoidal operations and had left the wound wide open. He considered that the complications arose only when the wound was partially closed at operation.

Dr. H. M. Jay (Adelaide) mentioned that the conditions described seemed to be more severe than the average. He would like to know whether it was not better to do two operations, at the first one merely to procure drainage and at the second to complete the removal of the ethmoidal and frontal sinuses. He asked whether any of those present had used Killian's long speculum to evacuate the pus of a cerebral abscess. He had had good results in rupture of the orbital plate by procuring drainage by the intranasal route, but this condition could not be classified with those of Dr. Marsh's patients.

Dr. A. B. K. Watkins (Newcastle) remarked that some of the patients did well and others badly and it was difficult to form a prognosis. The difficulty in the diagnosis of cerebral complications was that the function of the frontal lobe was obscure and that so much damage was done before symptoms occurred. Neurologists said that the first symptom was a mental condition similar to that of general paralysis of the insane, but he had never seen this. He had noted that the patients were drowsy and lay still, although they must have suffered pain. The second symptom was the diminution of the superficial abdominal reflexes on the opposite side of the body. He had seen this in patients with frontal sinusitis, who afterwards had recovered without operation. The condition was probably due to an early encephalitis which cleared up. He had often seen this. The third symptom was weakness of the muscles on the opposite side of the body which he personally had never seen. Bianchi had described skew deviation, but he had seen this only once.

As soon as a frontal lobe abscess was diagnosed, he employed a method of treatment similar to the one adopted by Dr. Marsh. The patient had a pulse below 60 and was drowsy and no abdominal reflexes could be elicited for

days after the onset. He thought it better to leave the brain alone till later and to perform only a frontal and ethmoidal operation; the patient recovered, but he was not sure that the end result would be satisfactory. Eagleton in his book had pointed out that most frontal abscesses were discovered *post mortem*. In his patient the lesion might have been a quiescent brain abscess. Eagleton had stated that when an extradural abscess complicated frontal sinusitis, there was also an intradural collection of pus. It required courage on the part of the operator to explore further. He preferred the intranasal route to remove the anterior ethmoidal cells which were always diseased in these conditions, and considered that the anterior ethmoidal region was the starting point of the trouble. He performed an intranasal frontal and ethmoidal operation and gave his patients two frontal sinus bougies and taught them to pass these up into the sinus at intervals for months later to keep open the fronto-nasal duct. Many patients with acute frontal sinusitis in the early stages responded to a 4% solution of ephedrine, two drops being placed in the nose with the head thrown well back.

Dr. Graham Brown (Brisbane) stated that when confronted with these severe affections he often felt at a loss how to proceed. All those present seemed to agree that a radical operation was necessary, but Harmer did very little beyond making a small incision and inserting the small rubber tube down the naso-frontal duct, later replacing this tube by a larger one and so dilating the duct. Harmer had told him that he had not performed a radical frontal operation for twenty years. He would not perform Harmer's operation in these fulminating conditions, but he thought sometimes he might do too much and that interference with bone might cause necrosis. He had had two severe cases both of which had ended fatally; one was in a man, aged twenty-three, who had acute frontal sinusitis of nine days' standing. He had performed an intranasal operation, but three days later he had diagnosed frontal lobe abscess; at operation he had found and drained two loculi of pus in the frontal lobe; the patient died and *post mortem* a third loculus with a thickened lining had been found, which had probably been there for months before.

In the second case he had had an X ray picture taken first which had revealed old standing trouble in the nose; in the skiagram he had missed a small area of necrosis twelve and a half millimetres above the frontal sinus; he had performed the radical frontal operation with local anæsthesia, nerve blocking the anterior ethmoidal nerve and other branches of the fifth nerve. After the operation the patient had seemed well, but later had shown signs of brain involvement and of spreading osteitis of the frontal bone. At a further operation he had cut large flaps of the soft tissues and had removed the unhealthy bone up to the vertex; he had found an extradural abscess. At the *post mortem* examination he had found a small track of pus leading into the lateral ventricle He said that they did not get enough of these cases to become competent to deal with them.

Chronic Infection of the Maxillary Antrum in Adults.

Dr. Clive M. Eadie (Melbourne) submitted a paper in which he dealt with chronic infection of the maxillary antrum in adults. First of all he considered the chronic catarrhal type. He discussed the ætiology and described the symptoms. In regard to treatment he said that septal resection and retraction of an impacted end of a middle turbinate bone might suffice in the early stages. Later on an antrostomy might provide adequate drainage. Once there was a generalized hypertrophy of the mucosa with or without the formation of polypi, cure would be obtained only by a radical operation. It was unwise to perform septal resection prior to the antrum operation in the presence of gross chronic mucosal change of the antrum. The antral condition might be made to flare up and the inflammation might extend to the frontal sinus. In regard to chronic suppurative antral infection, Dr. Eadie emphasized the importance of dental infection in the ætiology. In the diagnosis X ray examination was of the greatest assistance. The radical operation was the only treatment likely to be successful. He had received replies from 75 patients to whom a *questionnaire* had been sent in regard to the results of operation; 61 had derived great benefit, 13 had thought that there was no improvement and one patient had said that she felt worse since the operation, but did not think the operation was the cause.

Dr. Eric Gutteridge (Melbourne) said that at the Children's Hospital, Carlton, they had treated patients with antral disease by two or three lavages and had obtained 80% of cures. To avoid having to induce anæsthesia three times they had recently employed a modified Mikulicz operation, with a large intranasal opening, and had obtained better results with 80% of permanent cures. He asked at what stage in the disease should a radical operation be performed instead of an intranasal one. He considered that when the skiagram revealed the presence of polypoid mucosa within the antrum the radical operation should be carried out, but there were many conditions between this advanced stage and the milder form. He regarded the antrum as the cesspit of the nose and the important point was that the frontal and ethmoidal cells drained into the antrum instead of into the nose, and that the opening of the antrum was too high in man alone of all the animals. Accordingly pus accumulated in the antrum and caused thickening of the mucous membrane. Drainage was fourfold: first, by the cilia of the epithelium, secondly, capillary drainage by the apposition of the middle turbinal bone to the lateral nasal wall, thirdly by gravity and fourthly by the suction which occurred during breathing, which was similar to that of the Sprengel air pump. He felt with Ballenger that the point of attack was the middle turbinal and the removal of its anterior end was the reason of their success in their series of children, although it sacrificed the drainage by capillary suction. In early stages two handkerchief people could be treated by intranasal operation; he did not believe in subsequent lavage, but employed only instillations and painting with silver nitrate solution. He did not agree that the radical operation should be done in every case. X ray treatment was of great value. X ray pictures were slightly cloudy. When the patient had no nasal polypi, he should be treated by the intranasal antrum operation and the removal of the anterior end of the middle turbinal bone. He would wait for six months and then if necessary perform the radical operation. Ballenger spoke of the vicious circle of the *hiatus semilunaris* and Hajek said that his patients who were cured, were those on whom he had performed an intranasal antroscopy.

Dr. Ernest Culpin (Brisbane) said that the matter of technique was one for each surgeon to decide for himself. With regard to the classification into two groups he thought that there was a third variety which was troublesome to treat and might come under the classification of generalized catarrhal trouble of the mucous membrane of the nose and its sinuses. In this form the mucous membrane was pale and atrophic, the sinus openings were free and the patients frequently suffered from asthma. The condition of the antrum was of secondary importance and operations performed on the antrum were futile. It had been said that many surgeons did not speak the truth about their results by radical operation. He considered that nasal operations would give way to more conservative treatment. In one of his patients the nose had been full of polypi, but the patient had refused operation; he had given him thyreoid tablets and two weeks later all trace of the polypi had disappeared. He considered that there was very probably a biochemical cause for these conditions. In regard to radiological examination, sometimes the interpretation was confusing. A skiagram taken of one of his patients had revealed one antrum clear and the other absolutely dull; at operation the clear one had been full of pus and the dull one had been healthy.

Dr. W. Kent Hughes (Melbourne) said that with regard to focal infection it was not his experience to find the antrum the cause, but only the teeth and tonsils. He performed the Caldwell-Luc operation. He did not open at once into the mucous membrane of the anterior wall of the antrum, but cracked a circular piece of bone of the anterior wall and removed this without damaging the mucous membrane. He then endeavoured to remove the whole of the mucous membrane in one piece. He did not like the use of cocaine during general anæsthesia.

In children he thought that the importance of antral trouble was often not realized. At times deafness in children did not improve after the adenoids and tonsils had been removed. The antrum should always be examined. He did not consider skiagrams very valuable. He wondered if Dr. Gutteridge's cured patient had remained cured. Before he had used diathermy he had done many radical antrum operations. He used 300 milliampères with one terminal over the malar eminence; if this did not succeed, he did the radical antrum operation which was not much more severe than the intranasal one.

Dr. R. S. Godsall (Sydney) said that for eleven years he had been doing few radical antrum operations unless the ethmoidal cells were grossly diseased, but if there was no ethmoidal trouble, he performed Canfield's intranasal operation by which he was certain of removing all the mucous membrane inside the antrum as well as making a large opening between the antrum and the nose. When failure followed the intranasal operation, this was due to leaving the mucous membrane of the antrum. If the ethmoid were diseased, he did the radical antrum and radical ethmoidal operations at the same time.

Dr. Eric Blashki (Sydney) said that at the Royal North Shore Hospital he had often found chronic antral trouble after the tonsils and adenoids had been treated. He performed the Mikulicz operation. The conditions generally cleared up in this way, but if they did not, he did the radical antrum operation. In some instances when the proof puncture was clear, the pathological examination of the lavage showed infection (the Watson-Williams method). With regard to lavage after operation, he considered that all the pus would not escape from the antrum, however large the opening into the nose were made, and a certain amount of lavage was necessary. He generally carried this out twice a week for a few weeks, but did as little lavage as possible. He stated that Harmer had said that he did not do the radical antrum operation very often, but preferred the intranasal operation. He thought that failure after antral operation was generally due to ethmoidal disease. He removed less bone than Dr. Eadie and tried to avoid after trouble with the teeth. He thought that it was important to remove all the mucous membrane of the antrum. He made the opening into the nose by submucous resection of the nasal mucous membrane from the antral side as advised by Hajek and then slit the mucous membrane with a knife. He considered stereoscopic X ray pictures by far the best.

Dr. H. M. Jay (Adelaide) said that all agreed that diagnosis of latent antral infection was very difficult. The type of case he had in mind was one in which there was a constant mild mucoid discharge from the nose without any gross signs to be seen in the nose. The skiagraphic picture was of no use and proof puncture revealed nothing, even when the Watson-Williams method was employed. The naso-pharyngoscope was of advantage under these circumstances. A rope of mucus around or swelling of the antral opening could be seen through it. He was glad that postoperative lavage was generally condemned, because he did not think it rational. He used a nasal douche for two or three days after operation for the general comfort of the patient and then employed Sprengel's pump to suck the antrum dry. This also caused congestion which was of advantage for repair. He agreed with Dr. Culpin's third type, but did not know how to deal with it.

Dr. W. Crosse (Brisbane) found that transillumination was useful if the light was varied and the patient was observed for a long while. This he considered quite as useful as X ray examination. He did not like the intranasal operation, as patients who had been treated in this way by other doctors, had often come to him for further treatment. He thought the best way to inspect the antrum was through the canine fossa. He removed all the mucous membrane; this was the secret of success in the operation. He employed lavage afterwards and so got good results. He had had a chronic nasal discharge and a radical antrum operation had cured it. In antral infection from a septic tooth there was often supraorbital pain and tenderness over the frontal sinus. He never packed the antrum after operation, but only packed the nose along the floor around the intranasal opening.

Dr. Arthur Murphy (Brisbane) had not found X ray examination of much use at the Children's Hospital in Brisbane. He carried out X ray examination and proof puncture according to the Watson-Williams technique as a routine, but placed most reliance on his clinical findings. He had seen an anæsthetist in London using a modification of Hewitt's airway apparatus and packing gauze around it. The soft palate was pushed upwards to block the naso-pharynx without placing gauze in the naso-pharynx. He did not agree with Dr. Eadie about not packing the antrum because in one of his patients there had been a dehiscence of the posterior antral wall and a plexus of veins had been exposed. He thought that packing was necessary in such circumstances.

Dr. H. J. Gray (Perth) considered that X ray examination was useful if properly carried out. In one type of antral trouble there was slight discomfort and proof puncture revealed a clear straw-coloured fluid. If the skiagram disclosed the mucous membrane to be very thin, the intranasal operation sometimes cured the condition, but when the cysts were seen in the skiagram developing in the mucous membrane, radical operation was necessary.

Dr. G. A. D. McArthur (Melbourne) said that if they had to depend on the X ray findings to decide between the radical and intranasal operation, it would be necessary to wait at least two weeks after an attack of acute sinusitis. An X ray picture taken during the acute stage gave the wrong impression. If a second picture were taken a month later, it would be found that many of the changes had subsided.

Dr. G. C. Scantlebury (Melbourne) said that he was surprised that Dr. Godsall was the only one to mention the importance of accompanying ethmoidal trouble. In gross antral disease for which the radical operation was admittedly necessary, he tried to do a complete ethmoidal operation at the same time by the antral route which gave a good exposure. He found radiological examination very helpful, especially if much trouble were taken, and personally he would not operate on the nasal sinuses without a previous X ray picture.

Dr. G. Halloran (Sydney) said that his results in treating antral trouble in children were not good, especially those who had bronchiectasis in combination with antral disease; he would like to know how to cure such patients.

Dr. Graham Brown (Brisbane) considered that the question of sinus suppuration was of national importance and was increasing in Australia as a result of the dry, dusty climate. He would not like to be without the help of radiography and had worked with the same radiologist for the last ten years and had examined each picture with him. He agreed that skiagrams taken during the acute stage were misleading, as those changes seen were only temporary. A skiagram of the sinuses affected with catarrh revealed swelling of the mucous membrane of the sinuses. One of his patients had a sort of cyst in the mucous membrane full of pus and this was confirmed at operation. He thought that antral trouble secondary to septic teeth was commoner than was generally supposed. He had never seen joint trouble following infection of the sinuses. The floor of the antrum was lower than that of the nose and therefore lavage was necessary; he used it three days after operation with satisfactory results. In his own case he had felt much more comfortable after this had been done.

Dr. G. A. Eadie, brother of Dr. Clive Eadie (Melbourne) said that his views were not necessarily those of his brother. With regard to Dr. Gutteridge's remarks about treating children, he did the intranasal operation first, as there were usually none of the gross changes that were found at times in the adult type of infection. With regard to Dr. Culpin's atrophic variety he said that the worst type of antral trouble was that associated with atrophic rhinitis and in this condition he thought that the antral affection was secondary to some general metabolic disturbance. He thought Dr. Kent Hughes's idea of cracking the bone of the anterior wall a good one. He had never seen trouble by using cocaine during general anæsthesia. X ray examination was decidedly of help. He thought that the laryngoscope as recommended by Dr. Jay was advantageous. He preferred saline lavage to the use of the

Sprengel pump after operation. He agreed with Dr. Crosse that transillumination was useful, though not as useful as X ray examination and transillumination together. It was advisable to anticipate postoperative hæmorrhage by packing, but it was not common and the cavity could always be packed later if necessary. He thought that the type of case mentioned by Dr. Gray in which proof puncture yielded yellow fluid, would respond to treatment by repeated proof punctures and later the intranasal operation, if necessary. He agreed with Dr. Scantlebury that the ethmoidal cells should be removed through the antrum if they were grossly diseased.

(To be continued.)

Books Received.

INCOMPATIBILITY IN PRESCRIPTIONS AND HOW TO AVOID IT TO WHICH IS ADDED A DICTIONARY OF INCOMPATIBILITIES, by Thomas Stephenson, D.Sc., Ph.C., F.R.S. (Edinburgh), F.C.S.; New Edition, Revised and Enlarged; 1929. Edinburgh: "The Prescriber" Offices. Demy 8vo., pp. 61. Price: 4s. 6d. net.

A SHORT PRACTICE OF MIDWIFERY FOR NURSES, by Henry Jellett, B.A., M.D. (Dublin), F.R.C.P.I.; Eighth Edition; 1929. London: J. and A. Churchill. Crown 8vo., pp. 510, with illustrations. Price: 8s. 6d. net.

THE CAUSES AND PREVENTION OF MATERNAL MORTALITY, by Henry Jellett, B.A., M.D. (Dublin), F.R.C.P.I.; 1929. London: J. and A. Churchill. Demy 8vo., pp. 307. Price: 15s. net.

THE AFTER-TREATMENT OF OPERATIONS: A MANUAL FOR PRACTITIONERS AND HOUSE SURGEONS, by P. Lockhart-Mummery, F.R.C.S. (England), M.A., M.B., B.C. (Cantab.); Fifth Edition; 1929. London: Baillière, Tindall and Cox. Crown 8vo., pp. 290, with illustrations. Price: 7s. 6d. net.

RADIUM AND ITS SURGICAL APPLICATIONS, by H. S. Souttar, D.M., M.Ch (Oxon.), F.R.C.S. (England); 1929. Royal 8vo., pp. 67, with illustrations. Price: 4s. 6d. net.

Diary for the Month.

OCT. 15.—Tasmanian Branch, B.M.A.: Council.
OCT. 15.—New South Wales Branch, B.M.A.: Executive and Finance Committee.
OCT. 15.—New South Wales Branch, B.M.A.: Organization and Science Committee.
OCT. 16.—Western Australian Branch, B.M.A.: Branch.
OCT. 22.—New South Wales Branch, B.M.A.: Medical Politics Committee.
OCT. 23.—Victorian Branch, B.M.A.: Council.
OCT. 24.—Section of Medical Literature and History, New South Wales Branch, B.M.A.
OCT. 25.—Queensland Branch, B.M.A.: Council.
OCT. 31.—New South Wales Branch, B.M.A.: Branch.
OCT. 31.—South Australian Branch, B.M.A.: Branch.

Medical Appointments.

Dr. Alfred John Spencer Cecil Roberts (B.M.A.) has been appointed Acting Government Medical Officer at Toowoomba, Queensland.

* * *

Dr. Algernon Hampden Murphy (B.M.A.) has been appointed Government Medical Officer at Tweed Heads, New South Wales.

* * *

Dr. John Granger (B.M.A.) has been appointed Government Medical Officer at Tottenham, New South Wales.

Medical Appointments Vacant, etc.

FOR announcements of medical appointments vacant, assistants, *locum tenentes* sought, etc., see "Advertiser," page xviii.

LAUNCESTON PUBLIC HOSPITAL: Two Resident Medical Officers (male).

Medical Appointments: Important Notice.

MEDICAL practitioners are requested not to apply for any appointment referred to in the following table, without having first communicated with the Honorary Secretary of the Branch named in the first column, or with the Medical Secretary of the British Medical Association, Tavistock Square, London, W.C.1.

BRANCH.	APPOINTMENTS.
NEW SOUTH WALES: Honorary Secretary, 21, Elizabeth Street, Sydney.	Australian Natives' Association. Ashfield and District United Friendly Societies' Dispensary. Balmain United Friendly Societies' Dispensary. Friendly Society Lodges at Casino. Leichhardt and Petersham United Friendly Societies' Dispensary. Manchester Unity Medical and Dispensing Institute, Oxford Street, Sydney. North Sydney Friendly Societies' Dispensary Limited. People's Prudential Assurance Company, Limited. Phœnix Mutual Provident Society.
VICTORIAN: Honorary Secretary, Medical Society Hall, East Melbourne.	All Institutes or Medical Dispensaries. Australian Prudential Association Proprietary, Limited. Mutual National Provident Club. National Provident Association. Hospital or other appointments outside Victoria.
QUEENSLAND: Honorary Secretary, B.M.A. Building, Adelaide Street, Brisbane.	Members accepting appointments as medical officers of country hospitals in Queensland are advised to submit a copy of their agreement to the Council before signing. Brisbane United Friendly Society Institute. Stannary Hills Hospital. Toowoomba Friendly Societies Medical Institute.
SOUTH AUSTRALIAN: Secretary, 207, North Terrace, Adelaide.	All Contract Practice Appointments in South Australia. Booleroo Centre Medical Club.
WESTERN AUSTRALIAN: Honorary Secretary, 65, Saint George's Terrace, Perth.	All Contract Practice Appointments in Western Australia.
NEW ZEALAND (WELLINGTON DIVISION): Honorary Secretary, Wellington.	Friendly Society Lodges, Wellington, New Zealand.

Medical practitioners are requested not to apply for appointments to positions at the Hobart General Hospital, Tasmania, without first having communicated with the Editor of THE MEDICAL JOURNAL OF AUSTRALIA, The Printing House, Seamer Street, Glebe, New South Wales.

Editorial Notices.

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